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ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE [1]

TO-NIGHT, for the first time in its history, the British Association meets in the ancient city of Leicester; and it now becomes my privilege to convey to you, Mr. Mayor, and to the citizens generally, an expression of our thanks for your kind invitation and for the hospitable reception which you have accorded to us.

Here in Leicester and last year in York the association has followed its usual custom of holding its annual meeting somewhere in the United Kingdom; but in 1905 the meeting was, as you know, held in South Africa. Now, having myself only recently come from the Cape, I wish to take this opportunity of saying that this southern visit of the association has, in my opinion, been productive of much good; wider interest in science has been created amongst colonists, juster estimates of the country and its problems have been formed on the part of the visitors, and personal friendships and interchange of ideas between thinking men in South Africa and at home have arisen which can not fail to have a beneficial influence on the social, political and scientific relations between these colonies and the mother country. We may confidently look for like results from the proposed visit of the association to Canada in 1909.

One is tempted to take advantage of the wide publicity given to words from this

[1] Leicester, 1907.

chair to speak at large in the cause of science, to insist upon the necessity for its wider inclusion in the education of our youth and the devotion of a larger measure of the public funds in aid of scientific research; to point to the supreme value of science as a means for the culture of those faculties which in man promote that knowledge which is power; and to show how dependent is the progress of a nation upon its scientific attainment.

But in recent years these truths have been prominently brought before the association from this chair; they have been exhaustively demonstrated by Sir William Huggins from the chair of the Royal Society, and now a special guild[2] exists for their enforcement upon the mind of the nation.

These considerations appear to warrant me in following the healthy custom of so many previous presidents—viz., of confining their remarks mainly to those departments of science with which the labors of their lives have been chiefly associated.

THE SCIENCE OF MEASUREMENT

Lord Kelvin in 1871 made a statement from the presidential chair of the association at Edinburgh as follows:

> Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than the looking for something new. But nearly all the grandest discoveries of science have been the reward of accurate measurement and patient, long-continued labor in the minute sifting of numerical results.

Besides the instances quoted by Lord Kelvin in support of that statement, we have perhaps as remarkable and typical an exemplification as any in Lord Rayleigh's long-continued work on the density of nitrogen which led him to the discovery of argon. We shall see presently that, true as Lord Kelvin's words are in regard to most fields of science, they are specially applicable as a guide in astronomy.

[2] The British Science Guild.

One of Clerk Maxwell's lectures in the Natural Philosophy Class at Marischall College, Aberdeen, when I was a student under him there, in the year 1859, ran somewhat as follows:

> A standard, as it is at present understood in England, is not a real standard at all; it is a rod of metal with lines ruled upon it to mark the yard, and it is kept somewhere in the House of Commons. If the House of Commons catches fire there may be an end of your standard. A copy of a standard can never be a real standard, because all the work of human hands is liable to error. Besides, will your so-called standard remain of a constant length? It certainly will change by temperature, it probably will change by age (that is, by the rearrangement or settling down of its component molecules), and I am not sure if it does not change according to the azimuth in which it is used. At all events, you must see that it is a very impractical standard—impractical because, if, for example, any one of you went to Mars or Jupiter, and the people there asked you what was your standard of measure, you could not tell them, you could not reproduce it, and you would feel very foolish. Whereas, if you told any capable physicist in Mars or Jupiter that you used some natural invariable standard, such as the wave-length of the D-line of sodium vapor, he would be able to reproduce your yard or your inch, provided that you could tell him how many of such wave-lengths there were in your yard or your inch, and your standard would be available anywhere in the universe where sodium is found.

That was the whimsical way in which Clerk Maxwell used to impress great principles upon us. We all laughed before we understood; then some of us understood and remembered.

Now the scientific world has practically adopted Maxwell's form of natural standard. It is true that it names that standard the meter; but that standard is not one millionth of the earth's quadrant in length, as it was intended to be; it is merely a certain piece of metal approximately of that length.

It is true that the length of that piece

of metal has been reproduced with more precision, and is known with higher accuracy in terms of many secondary standards, than is the length of any other standard in the world; but it is, after all, liable to destruction and to possible secular change of length. For these reasons it can not be scientifically described otherwise than as a piece of metal whose length at 0° C. at the epoch A.D. 1906 is = 1,553,164 times the wave-length of the red line of the spectrum of cadmium when the latter is observed in dry air at the temperature of 15° C. of the normal hydrogen-scale at a pressure of 760 mm. of mercury at 0° C.

This determination, recently made by methods based on the interference of light-waves and carried out by MM. Perot and Fabry at the International Bureau of Weights and Measures, constitutes a real advance in scientific metrology. The result appears to be reliable within one ten-millionth part of the meter.

The length of the meter, in terms of the wave-length of the red line in the spectrum of cadmium, had been determined in 1892 by Michelson's method, with a mean result in almost exact accordance with that just quoted for the comparisons of 1906; but this agreement (within one part in ten millions) is due in some degree to chance, as the uncertainty of the earlier determination was probably ten times greater than the difference between the two independent results of 1892 and 1906.

We owe to M. Guillaume, of the same International Bureau, the discovery of the remarkable properties of the alloys of nickel and steel, and from the point of view of exact measurement the specially valuable discovery of the properties of that alloy which we now call "invar." He has developed methods for treatment of wires made from this alloy which render more permanent the arrangement of their constituent molecules. Thus these wires, with their attached scales, may, for considerable periods of time and under circumstances of careful treatment, be regarded as nearly invariable standards. With proper precautions, we have found at the Cape of Good Hope that these wires can be used for the measurement of base lines of the highest geodetic precision with all the accuracy attainable by the older and most costly forms of apparatus; whilst with the new apparatus a base of 20 kilometers can be measured in less time and for less cost than one of a single kilometer with the older forms of measurement.

THE GREAT AFRICAN ARC OF MERIDIAN

In connection with the progress of geodesy, time only permits me to say a few words about the Great African arc on the thirtieth meridian, which it is a dream of my life to see completed.

The gap in the arc between the Limpopo and the previously executed triangulation in Rhodesia, which I reported to the Association at the Johannesburg meeting in 1905, has now been filled up. My own efforts, at 6,000 miles distance, had failed to obtain the necessary funds, but at Sir George Darwin's instance contributions were obtained from this association, from the Royal Society and others, to the extent of half the estimated cost; the remaining half was met by the British South Africa Company. But for Darwin's happy intervention, which enabled me to secure the services of Captain Gordon and his party before the Transvaal Survey Organization was entirely broken up, this serious gap in the great work would probably have long remained; for it is one thing to add to an existing undertaking of the kind, it is quite another to create a new organization for a limited piece of work.

Since then Colonel (now Sir William)

Morris has brought to a conclusion the reductions of the geodetic survey of the Transvaal and Orange River Colony, and his report is now in my hands for publication.

Dr. Rubin, under my direction, at the cost of the British South Africa Company, has carried the arc of meridian northwards to south latitude 9° 42′, so that we have now continuous triangulation from Cape L'Agulhas to within fifty miles of the southern end of Lake Tanganyika; that is to say, a continuous geodetic survey extending over twenty-five degrees of latitude.

It happens that, for the adjustment of the international boundary between the British Protectorate and the Congo Free State, a topographic survey is at the present moment being executed northward along the thirtieth meridian from the northern border of German East Africa. A proposal on the part of the Royal Society, the Royal Geographical Society, the British Association and the Royal Astronomical Society has been made to strengthen this work by carrying a geodetic triangulation through it along the thirtieth meridian, and thus adding 2½° to the African arc. These societies together guarantee 1,000*l.* towards the cost of the work, and ask for a like sum from government to complete the estimated cost. The topographic survey will serve as the necessary reconnaisance. The topographic work will be completed by the end of January next, and the four following months offer the best season of the year for geodetic operations in these regions.

There is a staff of skilled officers and men on the spot sufficient to complete the work within the period mentioned, and the Intercolonial Council of the Transvaal and Orange River Colony most generously offers to lend the necessary geodetic instruments. The work will have to be done sooner or later, but if another expedition has to be organized for the purpose the work will then cost from twice to three times the present amount. One can not, therefore, doubt that His Majesty's government will take advantage of the present offer and opportunity to vote the small sum required. This done, we can not doubt that the German government will complete the chain along the eastern side of Lake Tanganyika, which lies entirely within their territory. Indeed, it is no secret that the Berlin Academy of Sciences has already prepared the necessary estimates with a view to recommending action on the part of its government.

Captain Lyons, who is at the head of the survey of Egypt, assures me that preliminary operations towards carrying the arc southwards from Alexandria have been begun, and we have perfect confidence that in his energetic hands the work will be prosecuted with vigor. In any case the completion of the African arc will rest largely in his hands. That arc, if ever my dream is realized, will extend from Cape L'Agulhas to Cairo, thence round the eastern shore of the Mediterranean and the islands of Greece, and there meet the triangulation of Greece itself, the latter being already connected with Struve's great arc, which terminates at the North Cape in lat. 70° N. This will constitute an arc of 105° in length—the longest arc of meridian that is measurable on the earth's surface.

THE SOLAR PARALLAX

Much progress has been made in the exact measurement of the great fundamental unit of astronomy—the solar parallax.

Early in 1877 I ventured to predict[3]

[3] "The Determination of the Solar Parallax," *The Observatory*, Vol. I., p. 280.

that we should not arrive at any certainty as to the true value of the solar parallax from observations of transits of Venus, but that the modern heliometer applied to the measurement of angular distances between stars and the star-like images of minor planets would yield results of far higher precision.

The results of the observations of the minor planets Iris, Victoria and Sappho at their favorable oppositions in the years 1888 and 1889, which were made with the cooperation of the chief heliometer and meridian observatories, fully justified this prediction.[4] The sun's distance is now almost certainly known within one thousandth part of its amount. The same series of observations also yielded a very reliable determination of the mass of the moon.

The more recently discovered planet Eros, which in 1900 approached the earth within one third of the mean distance of the sun, afforded a most unexpected and welcome opportunity for redetermining the solar parallax—an opportunity which was largely taken advantage of by the principal observatories of the northern hemisphere. Unfortunately the high northern declination of the planet prevented its observation at the Cape and other southern observatories. So far as the results have been reduced and published[5] they give an almost exact accordance with the value of the solar parallax derived from the heliometer observations of the minor planets, Iris, Victoria and Sappho in 1888 and 1889.

But in 1931 Eros will approach the earth within one sixth part of the sun's mean distance, and the fault will rest with astronomers of that day if they do not succeed in determining the solar parallax within one ten-thousandth part of its amount.

To some of us who struggled so hard to arrive at a tenth part of this accuracy under the less favorable geometrical conditions that were available before the discovery of Eros, how enviable seems the opportunity!

And yet, if we come to think of it rightly, the true opportunity and the chief responsibility is ours, for *now* and not twenty years hence is the time to begin our preparation; *now* is the time to study the origin of those systematic errors which undoubtedly attach to some of our photographic processes; and then we ought to construct telescopes specially designed for the work. These telescopes should be applied to the charting of the stars near the path which Eros will describe at its opposition in 1931, and the resulting star-coordinates derived from the plates photographed by the different telescopes should be rigorously intercompared. Then, if all the telescopes give identical results for the star-places, we can be certain that they will record without systematic error the position of Eros. If they do not give identical results, the source of the errors must be traced.

The planet will describe such a long path in the sky during the opposition of 1931 that it is already time to begin the meridian observations which are necessary to determine the places of the stars that are to be used for determining the constants of the plates. It is desirable, therefore, that some agreement should be come to with respect to selection of these reference-stars, in order that all the principal meridian observatories in the world may take part in observing them.

I venture to suggest that a Congress of

[4] *Annals of the Cape Observatory*, Vol. VI., part 6, p. 29.

[5] *Monthly Notices R.A.S.*, Hinks, Vol. LXIV., p. 725; Christie, Vol. LXVII., p. 382.

Astronomers should assemble in 1908 to consider what steps should be taken with reference to the important opposition of Eros in 1931.

THE STELLAR UNIVERSE

And now to pass from consideration of the dimensions of our solar system to the study of the stars, or other suns, that surround us.

To the lay mind it is difficult to convey a due appreciation of the value and importance of star-catalogues of precision. As a rule such catalogues have nothing whatever to do with discovery in the ordinary sense of the word, for the existence of the stars which they contain is generally well known beforehand; and yet such catalogues are, in reality, by far the most valuable assets of astronomical research.

If it be desired to demarcate a boundary on the earth's surface by astronomical methods, or to fix the position of any object in the heavens, it is to the accurate star-catalogue that we must refer for the necessary data. In that case the stars may be said to resemble the trigonometrical points of a survey, and we are only concerned to know from accurate catalogues their positions in the heavens at the epoch of observation. But in another and grander sense the stars are not mere landmarks, for each has its own apparent motion in the heavens which may be due in part to the absolute motion of the star itself in space, or in part to the motion of the solar system by which our point of view of surrounding stars is changed.

If we desire to determine these motions and to ascertain something of the general conditions which produce them, if we would learn something of the dynamical conditions of the universe and something of the velocity and direction of our own solar system through space, it is to the accurate star-catalogues of widely separated epochs that we must turn for a chief part of the requisite data.

The value of a star-catalogue of precision for present purposes of cosmic research varies as the square of its age and the square of its accuracy. We can not alter the epoch of our observations, but we can increase their value fourfold by doubling their accuracy. Hence it is that many of our greater astronomers have devoted their lives chiefly to the accumulation of meridian observations of high precision, holding the view that to advance such precision is the most valuable service to science they could undertake, and comforted in their unselfish and laborious work only by the consciousness that they are preparing a solid foundation on which future astronomers may safely raise the superstructure of sound knowledge.

But since the extension of our knowledge of the system of the universe depends quite as much on past as on future research, it may be well, before determining upon a program for the future, to consider briefly the record of meridian observation in the past for both hemispheres.

THE COMPARATIVE STATE OF ASTRONOMY IN THE NORTHERN AND SOUTHERN HEMISPHERES

It seems probable that the first express reference to southern constellations in known literature occurs in the Book of Job (ix. 9): "Which maketh Arcturus, Orion and Pleiades, and the chambers of the south." Schiaparelli's strongly supported conjecture is that the expression "chambers of the south," taken with its context, signifies the brilliant stellar region from Canopus to α Centauri, which includes the Southern Cross and coincides with the most brilliant portion of the Milky Way.

About the year 750 B. C. (the probable date of the Book of Job) all these stars culminated at altitudes between 5° and 16° when viewed from the latitude of Judea; but now, owing to precessional change, they can only be seen in a like striking manner from a latitude about 12° further south.

The words of Dante have unquestionably originated the wonderful net of poetic fancy that has been woven about the asterism, which we now call Crux.

> To the right hand I turned, and fixed my mind
> On the other pole attentive, where I saw
> Four stars ne'er seen before save by the ken
> Of our first parents—Heaven of their rays
> Seemed joyous. O thou northern site! bereft
> Indeed, and widowed, since of these deprived.

All the commentators agree that Dante here referred to the stars of the Southern Cross.

Had Dante any imperfect knowledge of the existence of these stars, any tradition of their visibility from European latitudes in remote centuries, so that he might poetically term them the stars of our first parents?

Ptolemy catalogues them as 31, 32, 33 and 34 Centauri, and they are clearly marked on the Borgian globe described by Assemanus in 1790. This globe was constructed by an Arabian in Egypt: it bears the date 622 Hegira, corresponding with A. D. 1225, and it is possible that Dante may have seen it.

Amerigo Vespucci, as he sailed in tropical seas, apparently recognized in what we now call Crux the four luminous stars of Dante; for in 1501 he claimed to be the first European to have looked upon the stars of our first parents. His fellow-voyager, Andrea Corsali, wrote about the same time to Giuliano di Medici describing "the marvelous cross, the most glorious of all the celestial signs."

Thus much mysticism and romance have been woven about this constellation, with the result that exaggerated notions of its brilliancy have been formed, and to most persons its first appearance, when viewed in southern latitudes, is disappointing.

To those, however, who view it at upper culmination for the first time from a latitude a little south of the Canary Islands, and who at the same time make unconsciously a mental allowance for the absorption of light to which one is accustomed in the less clear skies of northern Europe, the sight of the upright cross, standing as if fixed to the horizon, is a most impressive one. I at least found it so on my first voyage to the Cape of Good Hope. But how much more strongly must it have appealed to the mystic and superstitious minds of the early navigators as they entered the unexplored seas of the northern tropic! To them it must have appeared the revered image of the cross pointing the way on their southward course—a symbol and sign of hope and faith on their entry to the unknown.

The first general knowledge of the brighter stars of the southern hemisphere we owe to Frederick de Hautman, who commanded a fleet sent by the Dutch Government in 1595 to the Far East for the purpose of exploring Japan. Hautman was wrecked and taken prisoner at Sumatra, and whilst there he studied the language of the natives and made observations of the positions and magnitudes of the fixed stars of the southern hemisphere.[6]

Our distinguished countryman Halley visited St. Helena in 1677 for the purpose of cataloguing the stars of the southern hemisphere. He selected a station now marked Halley's Mount on the admiralty

[6] The resulting catalogue of 304 stars is printed as an appendix to Hautman's "Vocabulary of the Malay Language," published at Amsterdam in 1603.

chart of the island. I have visited the site, and the foundations of the observatory still remain. Halley's observations were much hindered by cloud. On his return to England, Halley in 1679 published his "Catalogus Stellarum Australium," containing the magnitudes, latitudes and longitudes of 341 stars, which, with the exception of seven, all belonged to the southern hemisphere.

But the first permanently valuable astronomical work in the southern hemisphere was done in 1751–52 by the Abbé de Lacaille. He selected the Cape of Good Hope as the scene of his labors, because it was then perhaps the only spot in the world situated in a considerable southern latitude which an unprotected astronomer could visit in safety, and where the necessary aid of trained artisans to erect his observatory could be obtained. Lacaille received a cordial welcome at the hands of the Dutch governor Tulbagh: he erected his observatory in Cape Town, made a catalogue of nearly 10,000 stars, observed the opposition of Mars, and measured a short arc of meridian all in the course of a single year. Through his labors the Cape of Good Hope became the birthplace of astronomy and geodesy in the southern hemisphere.

Bradley was laying the foundations of exact astronomy in the northern hemisphere at the time when Lacaille labored at the Cape. But Bradley had superior instruments to those of Lacaille and much longer time at his disposal. Bradley's work is now the basis on which the fair superstructure of modern astronomy of precision rests. His labors were continued by his successors at Greenwich and by a long series of illustrious men like Piazzi, Groombridge, Bessel, Struve and Argelander. But in the southern hemisphere the history of astronomy is a blank for seventy years from the days of Lacaille.

We owe to the establishment of the Royal Observatory at the Cape by an Order in Council of 1820 the first successful step towards the foundation of astronomy of high precision in the southern hemisphere.

Time does not permit me to trace in detail the labors of astronomers in the southern hemisphere down to the present day; and this is the less necessary because in a recent presidential address to the South African Philosophical Society[7] I have given in great part that history in considerable detail. But I have not there made adequate reference to the labors of Dr. Gould and Dr. Thome at Cordoba. To their labors, combined with the work done under Stone at the Cape, we owe the fact that for the epoch 1875 the meridian sidereal astronomy of the southern hemisphere is nearly as well provided for as that of the northern. The point I wish to make is that the facts of exact sidereal astronomy in the southern hemisphere may be regarded as dating nearly a hundred years behind those of the northern hemisphere.

THE CONSTITUTION OF THE UNIVERSE

It was not until 1718, when Edmund Halley, afterwards Astronomer Royal of England, read a paper before the Royal Society,[8] entitled "Considerations on the Change of the Latitudes of Some of the Principal Fixt Stars," that any definite facts were known about the constitution of the universe. In that paper Halley, who had been investigating the precession of the equinoxes, says:

> But while I was upon this enquiry I was surprised to find the latitudes of three of the principal stars in heaven directly to contradict the supposed

[7] *Trans. South African Phil. Soc.*, Vol. XIV., part 2.

[8] *Phil. Trans.*, 1718, p. 738.

greater obliquity of the Ecliptick, which seems confirmed by the latitudes of most of the rest.

This is the first mention in history of an observed change in the relative position of the so-called fixed stars—the first recognition of what we now call "proper motion."

Tobias Mayer, in 1760, seems to have been the first to recognize that if our sun, like other stars, has motion in space, that motion must produce apparent motion amongst the surrounding stars; for in a paper to the Göttingen Academy of Sciences he writes:

If the sun, and with it the planets and the earth which we inhabit, tended to move directly towards some point in the heavens, all the stars scattered in that region would seem to gradually move apart from each other, whilst those in the opposite quarter would mutually approach each other. In the same manner one who walks in the forest sees the trees which are before him separate, and those that he leaves behind approach each other.

No statement of the matter could be more clear; but Mayer, with the meager data at his disposal, came to the conclusion that "the motions of the stars are not governed by the above or any other common law, but belong to the stars themselves."

Sir William Herschel, in 1783, made the first attempt to apply, with any measure of success, Mayer's principle to a determination of the direction and amount of the solar motion in space.[9] He derived, as well as he could from existing data, the proper motions of fourteen stars, and arrived by estimation at the conclusion that the sun's motion in space is nearly in the direction of the star λ Herculis, and that 80 per cent. of the apparent motions of the fourteen stars in question could be assigned to this common origin.

This conclusion rests in reality upon a very slight basis, but the researches of subsequent astronomers show that it was an amazing accidental approach to truth—indeed, a closer approximation than Herschel's subsequent determinations of 1805 and 1806, which rested on wider and better data.[10]

Consider for a moment the conditions of the problem. If all the stars except our sun were at rest in space, then, in accordance with Mayer's statement, just quoted, all the stars would have apparent motions on great circles of the sphere away from the apex and towards the antapex of the solar motion. That is to say, if the position of each star of which the apparent motion is known was plotted on the surface of a sphere and a line with an arrow-head drawn through each star showing the direction of its motion on the sphere, then it should be possible to find a point on the sphere such that a great circle drawn from this point through any star would coincide with the line of direction of that star's proper motion. The arrow-heads would all point to that intersection of the great circles which is the antapex of the solar motion, and the other point of intersection of the great circles would be the apex, that is to say, the direction of the sun's motion in space.

But as the apparent stellar motions are small and only determinable with a considerable percentage of error, it would be impossible to find any point on the sphere such that every great circle passing through it and any particular star, would in every case be coincident with the observed direction of motion of that star.

Such discordances would, on our original assumption, be due to errors of observation, but in reality much larger discordances will occur, which are due to the fact that the other stars (or suns) have independent motions of their own in space.

[9] *Phil. Trans.*, 1783, p. 247.

[10] *Phil. Trans.*, 1805, p. 233; 1806, p. 205.

This at once creates a new difficulty, viz., that of defining an absolute locus in space. The human mind may exhaust itself in the effort, but it can never solve the problem. We can imagine, for example, the position of the sun at any moment to be defined with reference to any number of surrounding stars, but by no effort of imagination can we devise means of defining the *absolute* position of a body in space without reference to surrounding material objects. If, therefore, the referring objects have unknown motions of their own, the rigor of the definition is lost.

What we call the observed proper motion of a star has three possible sources of origin:

1. The *parallactic motion*, or the effect of our sun's motion through space, whereby our point of view of surrounding celestial objects is changed.

2. The *peculiar* or particular motion of the star, *i. e.*, its own *absolute* motion in space.

3. That part of the observed or tabular motion which is due to inevitable error of observation.

In all discussions of the solar motion in space, from that of Herschel down till a recent date, it has been assumed that the peculiar motions of the stars are arranged at random, and may therefore be considered zero in the mean of a considerable number of them. It is then possible to find such a value for the precession, and such a common apex for the solar motion as shall leave the residual peculiar motions of the stars under discussion to be in the mean = zero. That is to say, we refer the motion of the sun in space to the center of gravity of all the stars considered in the discussion, and regard that center of gravity as immovable in space.

In order to proceed rigorously, and especially to determine the amount as well as the direction of the sun's motion in space, we ought to know the parallax of every star employed in the discussion, as well as its proper motion. In the absence of such data it has been usual to start from some such assumption as the following: the stars of a particular magnitude are roughly at the same distance; those of different classes of magnitude may be derived from the hypothesis that on the average they have all equal absolute luminosity.

The assumption is not a legitimate one—

1. Because of the extreme difference in the absolute luminosity of stars.

2. Because it implies that the average absolute luminosity of stars is the same in all regions of space.

The investigation has been carried out by many successive astronomers on these lines with fairly accordant results as to the position of the solar apex, but with very unsatisfactory results as to the distances of the fixed stars.[11] In order to judge how far the magnitude (or brightness) of a star is an index of its probable distance, we must have evidence from direct determinations of stellar parallax.

STELLAR PARALLAX

To extend exact measurement from our own solar system to that of other suns

[11] Argelander, *Mém. présentés à l'Acad. Imp. des Sciences St. Pétersbourg*, tome III.; Lundahl, *Astron. Nachrichten*, **398**, 209; Argelander, *Astron. Nachrichten*, **398**, 210; Otto Struve, *Mém. Acad. des Sciences St. Pétersbourg*, VI^e série, Math. et Phys., tome III., p. 17; Galloway, *Phil. Trans.*, 1847, p. 79; Mädler, *Dorpat Observations*, Vol. XIV., and *Ast. Nach.*, **566**, 213; Airy, *Mém. R.A.S.*, Vol. XXVIII., p. 143; Dunkin, *Mem. R.A.S.*, Vol. XXXII., p. 19; Stone, *Monthly Notices R.A.S.*, Vol. XXIV., p. 36; De Ball, inaugural dissertation, Bonn, 1877; Rancken, *Astron. Nachrichten*, **2482**, 149; Bischoff, inaugural dissertation, Bonn, 1884; Ludwig Struve, *Mém. Acad. St. Pétersbourg*, VII^e série, tome XXXV., No. 3.

and other systems may be regarded as the supreme achievement of practical astronomy. So great are the difficulties of the problem, so minute the angles involved, that it is but in comparatively recent years that any approximate estimate could be formed of the true parallax of any fixed star. Bradley felt sure that if the star γ Draconis had a parallax of 1″ he would have detected it. Henderson by "the minute sifting of the numerical results" of his own meridian observations of α Centauri, made at the Cape of Good Hope in 1832–33, first obtained certain evidence of the measurable parallax of any fixed star. He was favored in this discovery by the fact that the object he selected happened to be, so far as we yet know, the nearest sun to our own. Shortly afterwards Struve obtained evidence of a measurable parallax for α Lyræ and Bessel for 61 Cygni. Astronomers hailed with delight this bursting of the constraints which our imperfect means imposed on research. But for the great purposes of cosmical astronomy what we are chiefly concerned to know is not what is the parallax of this or that particular star, but rather what is the average parallax of a star having a particular magnitude and proper motion. The prospect of even an ultimate approximate attainment of this knowledge seemed remote. The star α Lyræ is one of the brightest in the heavens; the star 61 Cygni one that had the largest proper motion known at the time; whilst α_2 Centauri is not only a very bright star, but it has also a large proper motion. The parallaxes of these stars must therefore in all probability be large compared with the parallax of the average star; but yet to determine them with approximate accuracy long series of observations by the greatest astronomers and with the finest instruments of the day seemed necessary.

Subsequently various astronomers investigated the parallaxes of other stars having large proper motions, but it was only in 1881, at the Cape of Good Hope, that general research on stellar parallax was instituted.[12] Subsequently at Yale and at the Cape of Good Hope the work was continued on cosmical lines with larger and improved heliometers.[13] By the introduction of the reversing prism and by other practical refinements the possibilities of systematic error were eliminated, and the accidental errors of observation reduced within very small limits.

These researches brought to light the immense diversity in the absolute luminosity and velocity of motion of different stars. Take the following by way of example:

Our nearest neighbor amongst the stars, α_2 Centauri, has a parallax of 0″.76, or is distant about 4⅓ light-years. Its mass is independently known to be almost exactly equal to that of our sun; and its spectrum being also identical with that of our sun, we may reasonably assume that it appears to us of the same magnitude as would our sun if removed to the distance of α_2 Centauri.

But the average star of the same apparent magnitude as α_2 Centauri was found to have a parallax of only 0″.10, so that either α_2 Centauri or our sun, if removed to a distance equal to that of the average fixed star of the first magnitude, would appear to us but little brighter than a star of the fifth magnitude.

Again, there is a star of only 8½ magnitude[14] which has the remarkable annual proper motion of nearly 8¾ seconds of arc —one of those so-called runaway stars—

[12] *Mem. R.A.S.*, Vol. XLVIII.

[13] *Annals of the Cape Observatory*, Vol. VIII., part 2, and *Trans. Astron. Observatory of Yale University*, Vol. I.

[14] Gould's Zones, V^h 243.

which moves with a velocity of 80 miles per second at right angles to the line of sight (we do not know with what velocity in the line of sight). It is at about the same distance from us as Sirius, but it emits but one ten-thousandth part of the light-energy of that brilliant star. Sirius itself emits about thirty times the light-energy of our sun, but it in turn sinks into insignificance when compared with the giant Canopus, which emits at least 10,000 times the light-energy of our sun.

Truly "one star differs from another star in glory." Proper motion rather than apparent brightness is the truer indication of a star's probable proximity to the sun. Every star of considerable proper motion yet examined has proved to have a measurable parallax.

This fact at once suggests the idea, Why should not the apparent parallactic motions of the stars, as produced by the sun's motion in space, be utilized as a means of determining stellar parallax?

SECULAR PARALLACTIC MOTION OF STARS

The strength of such determinations, unlike those made by the method of annual parallax, would grow with time. It is true that the process can not be applied to the determination of the parallax of individual stars, because the peculiar motion of a particular star can not be separated from that part of its apparent motion which is due to parallactic displacement. But what we specially want is not to ascertain the parallax of the individual star, but the mean parallax of a particular group or class of stars, and for this research the method is specially applicable, provided we may assume that the peculiar motions are distributed at random, so that they have no systematic tendency in any direction; in other words, that the center of gravity of any extensive group of stars will remain fixed in space.

This assumption is, of course, but a working hypothesis, and one which from the paper on star-streaming communicated by Professor Kapteyn, of Groningen, to the Johannesburg meeting of the Association two years ago we already know to be inexact.[15] Kapteyn's results were quite recently confirmed in a remarkable way by Eddington,[16] using independent material discussed by a new and elegant method. Both results showed that, at least for extensive parts of space, there are a nearly equal number of stars moving in exactly opposite directions. The assumption, then, that the mean of the peculiar motions is zero may, at least for these parts of space, be still regarded as a good working hypothesis.

Adopting an approximate position of the apex of the solar motion, Kapteyn resolved the observed proper motions of the Bradley stars into two components, viz., one in the plane of the great circle passing through the star and the apex, the other at right angles to that plane.[17] The former component obviously includes the whole of the parallactic motion; the latter is independent of it, and is due entirely to the real motions of the stars themselves. From the former the mean parallactic motion of the group is derived, and from the combination of the two components, the relation of velocity of the sun's motion to that of the mean velocity of the stars of the group.

As the distance of any group of stars found by the parallactic motion is expressed as a unit in terms of the sun's yearly motion through space, the velocity

[15] *Rep. Brit. Assoc.*, 1905, p. 257.

[16] *Monthly Notices R.A.S.*, Vol. LXVII., p. 34.

[17] *Publications Astron. Laboratory Groningen*, Nos. 7 and 9.

of this motion is one of the fundamental quantities to be determined. If the mean parallax of any sufficiently extensive group or class of stars was known we should have at once means for a direct determination of the velocity of the sun's motion in space; or if, on the other hand, we can by independent methods determine the sun's velocity, then the mean parallax of any group of stars can be determined.

DETERMINATION OF STELLAR MOTION IN THE LINE OF SIGHT

Science owes to Sir William Huggins the application of Doppler's principle to the determination of the velocity of star-motion in the line of sight. The method is now so well known, and such an admirable account of its theory and practical development was given by its distinguished inventor from this chair at the Cardiff meeting in 1891, that further mention of that part of the matter seems unnecessary.

THE VELOCITY OF THE SUN'S MOTION IN SPACE

If by this method the velocities in the line of sight of a sufficient number of stars situated near the apex and antapex of the solar motion could be determined, so that in the mean it could be assumed that their peculiar motions would disappear, we have at once a direct determination of the required velocity of the sun's motion.

The material for this determination is gradually accumulating, and indeed much of it, already accumulated, is not yet published. But even with the comparatively scant material available, it now seems almost certain that the true value of the sun's velocity lies between 18 and 20 kilometers per second;[18] or, if we adopt the mean value, 19 kilometers per second, this would correspond almost exactly with a yearly motion of the sun through space equal to four times the distance of the sun from the earth.

Thus the sun's yearly motion being four times the sun's distance, the parallactic motion of stars in which this motion is unforeshortened must be four times their parallax. How this number varies with the amount of foreshortening is of course readily calculated. The point is that from the mean parallactic motion of a group of stars we are now enabled to derive at once its mean parallax.

This research has been carried out by Kapteyn for stars of different magnitudes. It leads to the result that the parallax of stars differing *five* magnitudes does *not* differ in the proportion of one to ten, as would follow from the supposition of equal luminosity of stars throughout the universe, but only in the proportion of about one to five.[19]

The same method can not be applied to groups of stars of different proper motions, and it is only by a somewhat indirect proof, and by calling in the aid of such reliable results of direct parallax determination as we possess, that the variation of parallax with proper motion could be satisfactorily dealt with.

THE MEAN PARALLAXES OF STARS OF DIFFERENT MAGNITUDE AND PROPER MOTION

As a final result Kapteyn derived an empirical formula giving the average parallax for stars of different spectral types, and of any given magnitude and proper motion. This formula was published at Groningen in 1901.[20] Within the

[18] *Kapteyn Ast. Nach.*, No. 3487, p. 108; and Campbell, *Astrophys. Journ.*, XIII., p. 80.

[19] *Astron. Nachrichten*, No. 3487, Table III.; and *Ast. Journ.*, p. 566.

[20] *Publications Astron. Laboratory Groningen*, No. 8, p. 24.

past few months the results of researches on stellar parallax, made under the direction of Dr. Elkin, at the Astronomical Observatory of Yale University, during the past thirteen years,[21] have been published, and they afford a most crucial and entirely independent check on the soundness of Kapteyn's conclusions.

COMPARISON GROUPS ARRANGED IN ORDER OF PROPER MOTION

No. of Stars	Proper Motion	Magnitude	Parallax		Yale-Kapteyn
			Yale	Kapteyn	
21	0.″14	3.8	0.″028	0.″026	+0.″002
39	0.49	6.3	.042	.055	— .013
45	0.59	6.7	.068	.060	+ .008
46	0.77	6.5	.047	.074	— .027
22	1.50	6.2	.118	.124	— .006

GROUPS ARRANGED IN ORDER OF MAGNITUDE

No. of Stars	Proper Motion	Magnitude	Parallax		Yale-Kapteyn
			Yale	Kapteyn	
10	0.″61	0.8	0.″103	0.″110	—0.″007
29	.53	3.8	.076	.075	+ .001
33	.63	5.6	.064	.070	— .006
34	.73	6.7	.055	.070	— .017
31	.68	7.6	.025	.061	— .036
36	.80	8.3	.056	.062	— .006

	No. of Stars	Proper Motion	Magnitude	Parallax		Yale-Kapteyn
				Yale	Kapteyn	
Spectral Type I.	13	0.″42	4.0	0.″076	0.″076	0.″000
Spectral Type II.	81	0.67	5.3	0.067	0.074	—0.007

In considering the comparison between the more or less theoretical results of Kapteyn and the practical determinations of Yale, we have to remember that Kapteyn's tables refer only to the means of groups of a large number of stars having on the average a specified magnitude and proper motion, whilst the latter are direct determinations affected by the accidental errors of the separate determinations and by such uncertainty as attaches to the unknown parallaxes of the comparison stars—parallaxes which we have supplied from Kapteyn's general tables.

The Yale results consist of the determination of the parallax of 173 stars, of which only ten had been previously known to Kapteyn and had been utilized by him. Dividing these results into groups we get the comparison given above.

These results agree in a surprisingly satisfactory way, having regard to the comparatively small number of stars in each group and the great range of parallax which we know to exist amongst individual stars having the same magnitude and proper motion. In the mean perhaps the tabular parallaxes are in a minute degree too large, but we have unquestionable proof from this comparison that our knowledge of stellar distances now rests on a solid foundation.

THE DISTRIBUTION OF VARIETIES OF LUMINOSITY OF STARS

But, besides the mean parallax of stars of a particular magnitude and proper motion, it is essential that we should know approximately what percentage of the stars of such a group have twice, three times, etc., the mean parallax of the group, and what percentage only one half, one third of that parallax, and so on. In principle, at least, this frequency-law may be obtained by means of the directly determined parallaxes. For the stars of which we have reliable determinations we can compare these true parallaxes with the *mean* parallax of stars having corresponding magnitude and proper motion, and this comparison will lead to a knowledge of the frequency-law required. It is true that, owing to the scarcity of material at present available, the determination of the fre-

[21] *Trans. Astron. Observatory of Yale Univ.*, Vol. II., part 1.

quency-law is not so strong as may be desirable, but further improvement is simply a question of time and the augmentation of parallax-determination.

Adopting provisionally the frequency-law found in this way by Kapteyn,[22] we can localize all the stars in space down to about the ninth magnitude.

Take, for example, the stars of magnitude 5.5 to 6.5. There are about 4,800 of these stars in the whole sky. According to Auwers-Bradley, about 9½ per cent. of these stars, or some 460 in all, have proper motions between 0″.04 and 0″.05. Now, according to Kapteyn's empiric formula, whose satisfactory agreement with the Yale results has just been shown, the mean parallax of such stars is almost exactly 0″.01. Further, according to his frequency-law, 29 per cent. of the stars have parallaxes between the *mean* value and double the mean value; 6 per cent. have parallaxes between twice and three times the mean value; 1½ per cent. between three and four times the mean value. Therefore of our 460 stars 133 will have parallaxes between 0″.01 and 0″.02, twenty-eight between 0″.02 and 0″.03, seven between 0″.03 and 0″.04, and so on.

Localizing in the same way the stars of the sixth magnitude having other proper motions, and then treating the stars of the first magnitude, second magnitude, third magnitude, and so on to the ninth magnitude in the same way, we finally locate all these stars in space.[23]

It is true we have not localized the individual stars, but we know approximately and within certain limits of magnitude the number of stars at each distance from the sun.

Thus the apparent brightness and the distance being known we have the means of determining the light-energy or absolute *luminosity* of the stars, *provided it can be assumed that light does not suffer any extinction in its passage through interstellar space.*

On this assumption Kapteyn was led to the following results, viz., that within a sphere the radius of which is 560 light-years (a distance which corresponds with that of the average star of the ninth magnitude) there will be found:

1	star	giving from	100,000	to	10,000	times	the light	of our	sun.
26	stars	"	10,000	"	1,000	"	"	"	
1,300	"	"	1,000	"	100	"	"	"	
22,000	"	"	100	"	10	"	"	"	
140,000	"	"	10	"	1	"	"	"	
430,000	"	"	1	"	0.1	"	"	"	
650,000	"	"	0.1	"	0.01	"	"	"	

THE DENSITY OF STELLAR DISTRIBUTION AT DIFFERENT DISTANCES FROM OUR SUN

Consider, lastly, the distribution of stellar density, that is, the number of stars contained in the unit of volume.

We can not determine *absolute* star-density, because, for example, some of the stars which we know from their measured parallaxes to be comparatively near to us are in themselves so little luminous that if removed to even a few light-years greater distance they would appear fainter than the ninth magnitude, and so fall below the magnitude at which our data at present stop.

But if we assume that intrinsically faint and bright stars are distributed in the

[22] *Publications Astron. Lab. Groningen,* No. 8, p. 23.

[23] *Ibid.*, No. 11, Table II.

same proportion in space, it will be evident that the comparative richness of stars in any part of the system will be the same as the comparative richness of the same part of the system in stars of a particular luminosity. Therefore, as we have already found the arrangement in space of the stars of different degrees of luminosity, and consequently their number at different distances from the sun, we must also be able to determine their relative density for these different distances.

Kapteyn finds in this way that, starting from the sun, the star-density (*i. e.*, the number of stars per unit volume of space) is pretty constant till we reach a distance of some 200 light-years. Thence the density gradually diminishes till, at about 2,500 light-years, it is only about *one fifth* of the density in the neighborhood of the sun.[24] This conclusion must, however, be regarded as uncertain until we have by independent means been enabled to estimate the absorption of light in its course through interstellar space, and obtained proof that the ratio of intrinsically faint to bright stars is constant throughout the universe.

Thus far Kapteyn's researches deal with the stellar universe as a whole; the results, therefore, represent only the *mean* conditions of the system. The further development of our knowledge demands a like study applied to the several portions of the universe separately. This will require much more extensive material than we at present possess.

As a first further approximation the investigation will have to be applied separately to the Milky Way and the parts of the sky of higher galactic latitude. The velocity and direction of the sun's motion in space may certainly be treated as constants for many centuries to come, and these constants may be separately determined from groups of stars of various regions, various magnitudes, various proper motions, and various spectral types. If these constants as thus separately determined are different, the differences which are not attributable to errors of observation must be due to a common velocity or direction of motion of the group or class of star to which the sun's velocity or direction is referred. Thus, for example, the sun's velocity as determined by spectroscopic observations of motion in the line of sight, appears to be sensibly smaller than that derived from fainter stars. The explanation appears to be that certain of the brighter stars form part of a cluster or group of which the sun is a member, and these stars tend to some extent to travel together. For these researches the existing material, especially that of the determination of velocities in the line of sight, is far too scanty.

Kapteyn has found that stars whose proper motions exceed 0″.05 are not more numerous in the Milky Way than in other parts of the sky;[25] in other words, if only the stars having proper motions of 0″.05 or upwards were mapped there would be no aggregation of stars showing the existence of a Milky Way.

The proper motions of stars of the second spectral type are, as a rule, considerably larger than those of the first type; but Kapteyn comes to the conclusion that this difference does not mean a real difference of velocity, but only that the second-type stars have a smaller luminosity, the mean difference between the two types amounting to $2\frac{1}{2}$ magnitudes.[26]

THE FUTURE COURSE OF RESEARCH

In the last address delivered from this chair on an astronomical subject, Sir Wil-

[24] *Publications Astron. Lab. Groningen*, No. 11.

[25] *Verl. Kn. Akad. Amsterdam*, January, 1893.

[26] *Ibid.*, April, 1892.

liam Huggins, in 1891, dealt so fully with the chemistry of the stars that it seemed fitting on the present occasion to consider more especially the problem of their motion and distribution in space, as it is in this direction that the most striking advances in our knowledge have recently been made. It is true that since 1891 great advances have also been made in our detailed knowledge of the chemistry of the sun and stars. The methods of astro-spectrography have been greatly improved, the precision of the determination of motion in the line of sight greatly enhanced, and many discoveries made of those close double stars, ordinarily termed spectroscopic doubles, the study of which seems destined to throw illustrative light upon the probable history of the development of systems from the original nebular condition to that of more permanent systems.

But the limitations of available time prevent me from entering more fully into this tempting field, more especially as it seems desirable, in the light of what has been said, to indicate the directions in which some of the astronomical work of the future may be most properly systematized. There are two aspects from which this question may be viewed. The first is the more or less immediate extension of knowledge or discovery; the second the fulfilment of our duty, as astronomers, to future generations. These two aspects should never be entirely separated. The first, as it opens out new vistas of research and improved methods of work, must often serve as a guide to the objects of the second. But the second is to the astronomer the supreme duty, viz., to secure for future generations those data the value of which grows by time.

As the result of the Congress of Astronomers held at Paris in 1887 some sixteen of the principal observatories in the world are engaged, as is well known, in the laborious task, not only of photographing the heavens, but of measuring these photographs and publishing the *relative* positions of the stars on the plates down to the eleventh magnitude. A century hence this great work will have to be repeated, and then, if we of the present day have done our duty thoroughly, our successors will have the data for an infinitely more complete and thorough discussion of the motions of the sidereal system than any that can be attempted to-day. But there is still needed the accurate meridian observation of some eight or ten stars on each photographic plate, so as to permit the conversion of the *relative* star-places on the plate into *absolute* star-places in the heavens. It is true that some of the astronomers have already made these observations for the reference stars of the zones which they have undertaken. But this seems to be hardly enough. In order to coordinate these zones, as well as to give an accuracy to the *absolute* positions of the reference stars corresponding with that of the *relative* positions, it is desirable that this should be done for *all* the reference stars in the sky by several observatories. The observations of well-distributed stars by Kustner at Bonn present an admirable instance of the manner in which the work should be done. Several observatories in each hemisphere should devote themselves to this work, employing the same or other equally efficient means for the elimination of sources of systematic error depending on magnitude, etc., and it is of far more importance that we should have, say, two or three observations of each star at three different observatories than two or three times as many observations of each star made at a single observatory.

The southern can not boast of a richness of instrumental and personal equipment

comparable with that of the northern hemisphere, and consequently one welcomes with enthusiasm the proposal on the part of the Carnegie Institution to establish a meridian observatory in a suitable situation in the southern hemisphere. Such an observatory, energetically worked, with due attention to all necessary precautions for the exclusion of systematic errors, would conduce more than anything else to remedy in some degree that want of balance of astronomical effort in the two hemispheres to which allusion has already been made. But in designing the program of the work it should be borne in mind that the proper duty of the meridian instrument in the present day is no longer to determine the positions of all stars down to a given order of magnitude, but to determine the positions of stars which are geometrically best situated and of the most suitable magnitude for measurement on photographic plates, and to connect these with the fundamental stars. For this purpose the working list of such an observatory should include only the fundamental stars and the stars which have been used as reference stars for the photographic plates.

Such a task undertaken by the Carnegie Observatory, by the Cape, and if possible by another observatory in the southern hemisphere, and by three observatories in the northern, would be regarded by astronomers of the future as the most valuable contribution that could be made to astronomy of the present day. Taken in conjunction with the astrographic survey of the heavens now so far advanced, it is an opportunity that if lost can never be made good; a work that would grow in value year by year as time rolls on, and one that would ever be remembered with gratitude by the astronomers of the future.

But for the solution of the riddle of the universe much more is required. Besides the proper motions, which would be derived from the data just described, we need for an ideal solution to know the velocity in the line of sight, the parallax, the magnitude, and the spectrum-type of every star.

The broad distinction between these latter data and the determination of proper motion is this, that whereas the observations for proper motion increase in value as the square of their age, those for velocity in the line of sight, parallax, magnitude, and type of spectrum may, for the broader purposes of cosmical research, be made at any time without loss of value. We should therefore be most careful not to sacrifice the interests of the future by immediate neglect of the former for the latter lines of research. The point is that those observatories which undertake this meridian work should set about it with the least possible delay, and prosecute the program to the end with all possible zeal. Three observatories in each hemisphere should be sufficient; the quality of the work should be of the best, and quality should not be sacrificed for speed of work.

But the sole prosecution of routine labor, however high the ultimate object, would hardly be a healthy condition for the astronomy of the immediate future. The sense of progress is essential to healthy growth, the desire to know must in some measure be gratified. We have to test the work that we have done in order to be sure that we are working on the right lines, and new facts, new discoveries, are the best incentives to work.

For these reasons Kapteyn, in consultation with his colleagues in different parts of the world, has proposed a scheme of research which is designed to afford within a comparatively limited time a great augmentation of our knowledge. The principle on which his program is based is that adequate data as to the proper mo-

tions, parallaxes, magnitudes, and the type of spectrum of stars situated in limited but symmetrically distributed areas of the sky, will suffice to determine many of the broader facts of the constitution of the universe. His proposals and methods are known to astronomers and need not therefore be here repeated. In all respects save one these proposals are practical and adequate, and the required cooperation may be said to be already secured—the exception is that of the determination of motion in the line of sight.

All present experience goes to show that there is no known satisfactory method of determining radial velocity of stars by wholesale methods, but that such velocities must be determined star by star. For the fainter stars huge telescopes and spectroscopes of comparatively low dispersion must be employed. On this account there is great need in both hemispheres of a huge reflecting telescope—six to eight feet in aperture—devoted almost exclusively to this research. Such a telescope is already in preparation at Mount Wilson, in America, for use in the northern hemisphere. Let us hope that Professor Pickering's appeal for a large reflector to be mounted in the southern hemisphere will meet with an adequate response, and that it will be devoted there to this all-important work.

CONCLUSION

The ancient philosophers were confident in the adequacy of their intellectual powers alone to determine the laws of human thought and regulate the actions of their fellow men, and they did not hesitate to employ the same unsupported means for the solution of the riddle of the universe. Every school of philosophy was agreed that some object which they could see was a fixed center of the universe, and the battle was fought as to what that center was. The absence of facts, their entire ignorance of methods of exact measurement, did not daunt them, and the question furnished them a subject of dispute and fruitless occupation for twenty-five centuries.

But astronomers now recognize that Bradley's meridian observations at Greenwich, made only one hundred and fifty years ago, have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They have learned the lesson that human knowledge in the slowly developing phenomena of sidereal astronomy must be content to progress by the accumulating labors of successive generations of men; that progress will be measured for generations yet to come more by the amount of honest, well-directed, and systematically discussed observation than by the most brilliant speculation; and that, in observation, concentrated systematic effort on a special thoughtfully selected problem will be of more avail than the most brilliant but disconnected work.

By these means we shall learn more and more of the wonders that surround us, and recognize our limitations when measurement and facts fail us.

Huggins's spectroscope has shown that many nebulæ are not stars at all; that many well-condensed nebulæ, as well as vast patches of nebulous light in the sky, are but inchoate masses of luminous gas. Evidence upon evidence has accumulated to show that such nebulæ consist of the matter out of which stars (*i. e.*, suns) have been and are being evolved. The different types of star spectra form such a complete and gradual sequence (from simple spectra resembling those of nebulæ onwards through types of gradually increasing complexity) as to suggest that we have before us, written in the cryptograms of these

spectra, the complete story of the evolution of suns from the inchoate nebula onwards to the most active sun (like our own), and then downward to the almost heatless and invisible ball. The period during which human life has existed on our globe is probably too short—even if our first parents had begun the work—to afford observational proof of such a cycle of change in any particular star; but the fact of such evolution, with the evidence before us, can hardly be doubted. I most fully believe that, when the modifications of terrestrial spectra under sufficiently varied conditions of temperature, pressure, and environment have been further studied, this conclusion will be greatly strengthened. But in this study we must have regard also to the spectra of the stars themselves. The stars are the crucibles of the Creator. There we see matter under conditions of temperature and pressure and environment, the variety of which we can not hope to emulate in our laboratories, and on a scale of magnitude beside which the proportion of our greatest experiment is less than that of the drop to the ocean. The spectroscopic astronomer has to thank the physicist and the chemist for the foundation of his science, but the time is coming—we almost see it now—when the astronomer will repay the debt by wide-reaching contributions to the very fundamenta of chemical science.

By patient, long-continued labor in the minute sifting of numerical results, the grand discovery has been made that a great part of space, so far as we have visible knowledge of it, is occupied by two majestic streams of stars traveling in opposite directions. Accurate and minute measurement has given us some certain knowledge as to the distances of the stars within a certain limited portion of space, and in the cryptograms of their spectra has been deciphered the amazing truth that the stars of both streams are alike in design, alike in chemical constitution, and alike in process of development.

But whence have come the two vast streams of matter out of which have been evolved these stars that now move through space in such majestic procession?

The hundreds of millions of stars that comprise these streams, are they the sole ponderable occupants of space? However vast may be the system to which they belong, that system itself is but a speck in illimitable space; may it not be but one of millions of such systems that pervade the infinite?

We do not know.

"Canst thou by searching find out God? canst thou find out the Almighty unto perfection?"

David Gill

SCIENTIFIC BOOKS

A Text-book of Electro-Chemistry. By Max Le Blanc, Professor in the University of Leipzig. Translated from the Fourth German Edition by W. R. Whitney, Ph.D., Director of the Research Laboratory of the General Electric Company, and John W. Brown, Director of the Research and Battery Laboratory of the National Carbon Company. 8vo, pp. xiv + 338. Price, $2.60 net. New York: The Macmillan Company.

That two busy men, immersed to their eyes in solving the commercial problems of two great industrial corporations, should have the courage of their convictions to the extent of themselves translating this splendid text-book, is a most hopeful sign of the times, whichever way it is regarded. What more could be wished, than that a text-book should originate within the classic precincts of a university, and be translated and sponsored by the heads of two commercial laboratories? And the book is worthy of its origin.

The earlier editions of Le Blanc's book are

so well known to electro-chemists, that but a brief allusion to the contents as a whole is necessary here. The well-balanced chapters deal with the fundamental principles, historical development, theory of electrolytic dissociation, migration of ions, conductance of electrolytes, electrical endosmose and electrostenolysis, electromotive force, electrolysis and polarization, a supplement on accumulators and an appendix describing the scheme of notation employed.

As to the way in which these are handled, the English is above criticism and the presentation is lucid and comprehensible to the last degree. There is nowhere the slightest chance for misunderstanding the writer's ideas, whether one accepts them as a finality or not. As a presentation of the fundamental facts and the prevalent theories of electrochemistry, the work is probably without an equal, certainly without a superior. As prices go, the book is much cheaper than usual—an additional recommendation, probably ascribable to the broad views and sound commercial instincts of the translators and publishers.

As to the plan of the work, the theory of electrolytic dissociation is followed consistently throughout. We regret to say, however, that although so praiseworthy in other respects, the form of statement in terms of the theory is not always free from objection, and to give the student an unbiased, absolutely unobjectionable idea, would need revision by the teacher. To illustrate: "*The value 13,700 calories then really represents the heat of dissociation of water*" (p. 134). The statement should certainly have been qualified by saying *electrolytic dissociation*, or even *ionization*, for *dissociation;* otherwise, the statement as it stands, is certainly incorrect.

A series of inconsistencies is caused by following in too uncritical a spirit the teachings of the dissociation theory. Thus, on page 94 we have:

The degree of dissociation of a substance in solution is equal to the ratio of its equivalent conductance in that solution to its equivalent conductance in a solution of infinite volume.

But on page 148, discussing the di-electric constants of solvents, we have:

From this fact it follows that it is inadmissible to draw a conclusion, as often has been done, regarding the degree of dissociation from the value of the equivalent conductance alone.

Such inconsistencies (and there are others of analogous character) are the chief defects of the book. The tenets of the dissociation theory are laid down with emphasis, usually in italics, as above, and then later the experimental facts which negative some of those statements are either not mentioned, or mentioned with very slight emphasis, or else freely admitted, and yet their full import and effect glossed over. There is in many cases an apparent willingness to admit facts contrary to the tenets of the dissociation theory, and yet such facts are not pushed to their full, legitimate conclusion.

All of this shows that the theories of electrochemistry are in a state of transition; even the teachers with the best of purposes to see all sides are sometimes staring at one side and blinking at the other. The perfectly judicial attitude of mind is, at present, difficult if not impossible to preserve. A few years will see a new era of electrochemical theory and teaching, wherein the student is nurtured as a plain eclectic, keen to see and quick to admit the truth wherever and in whatever guise he finds it.

Are we speaking of an unattainable millennium? We hope not.

Joseph W. Richards

Tropical Medicine. By Thomas W. Jackson, M.D.

The acquisition by this government within the last few years of the Philippine Islands, Puerto Rico and the Panama Canal District, and its necessary sanitary supervision over Cuba in its relation to yellow fever, have made the study of tropical diseases one of great interest and, especially for the physicians of the southern states, one of practical necessity, for as our knowledge of these so-called tropical

diseases increases, we find that not only are they to be found in the tropics, but also to a greater or less extent in our own country.

The book is divided into several parts, beginning, of course, with the usual introduction, but in this case including a discussion of tropical hygiene. The treatment of this subject denotes a familiarity with conditions as found in the tropics obtained only by experience, and if the advice given were followed, it would materially decrease the amount of sickness and the number of deaths occurring among those living there.

That portion of the book which treats of mosquitoes, though brief, is well worth studying for those likely to be brought into contact with either malaria or yellow fever.

The book is written in a narrative style, the usual text-book description of the subjects being given, but with sufficient personal experiences interspersed to lend an added interest to the subject under discussion.

The book is divided into three parts, the first part dealing with "Systemic Diseases (Chiefly Bacterial in Origin)." This section is mainly concerned with a discussion of the infectious diseases.

Under the heading of Cholera, the description of bacteriologic technic to be used for diagnostic purposes is faulty and not clear, and leaves the impression that a mere novice could make a diagnosis, whereas, as is well known, cases arise which offer great difficulty, from the presence of other spirilla giving similar reactions and only differentiated by agglutination or animal experiments. The author lays considerable stress on the use of the anti-cholera serum prepared by the Japanese, which, he says, has an anti-toxic action.

It is surprising to note, as is stated, that Haffkine's prophylactic had never been used as a curative agent for plague; its use for such a purpose would certainly seem contra-indicated. Under the same heading, the author recommends for the agglutination test that "the serum be diluted with normal salt solution to a proportion of 1:3." It is very doubtful whether agglutination obtained with such a dilution would be of any value.

The chapter on malaria is written in a more detailed way than those on the other diseases.

The author apparently believes in the infectious nature of beri-beri, accepting the work of Wright, though giving rather full abstracts of the report of Baron Takaki of the Japanese navy, who ascribes the marked decrease in the number of cases in the Japanese navy to a change of diet consisting of the addition of barley and an increased amount of meat to the usual rice diet.

In the chapter on yellow fever, considerable space is justly given to the work of Reed, Carroll, Agramonte and Lazear, composing the board appointed by the government for the study of yellow fever.

The second part is taken up with a discussion of diseases produced by animal parasites. The chapter on ankylostomiasis is excellent, the author here again detailing his own experiences. The remainder of this section is taken up with a discussion of filariasis, trypanosomiasis and those diseases produced by parasites peculiar to the tropics.

The third part treats of diseases of undetermined causation and of the skin. Under this heading is to be found a brief description of such diseases as acute febrile icterus, mycetoma, tropical splenomegaly and of some of the parasitic skin diseases.

At the end is a list of articles recommended for diagnostic purposes which would undoubtedly be of great value for any practising physician.

A perusal of the book would be of benefit to any one likely to come into relation with tropical diseases.

James W. Jobling

Rockefeller Institute for Medical Research

DISCUSSION AND CORRESPONDENCE

ON THE EFFECTS OF MAGNESIUM SULPHATE ON THE GROWTH OF SEEDLINGS

A recent issue of Science contains a letter from Professor Oscar Loew, which, for some unaccountable reason, is entitled "a correction." I have read the letter carefully several

times in an endeavor to find in it the promised "rectification," but without success. It is obvious, however, that, in the letter referred to, Professor Loew announced a particular text, then read into it miscellaneous matters that were off the subject and next proceeded to belabor the men of straw his imagination introduced to the reader.

Professor Loew began the letter as follows:

A statement on page 452 of SCIENCE of March 22 requires a rectification in the interest of the unprejudiced reader.

The sentence in question reads as follows:

"These results show conclusively that magnesium sulphate in proper dilution is beneficial to the growth of seedlings, and that any inhibitory effects are due to the presence of excessive amounts, thus controverting Loew's theory that magnesium salts when alone in solution are always injurious to plant growth."

This quotation from the abstract of Miss Burlingham's communication at the last meeting of the Biological Section of the American Chemical Society was followed, in Professor Loew's letter, by the remarks quoted below (1–6), to each of which I have appended a brief reply from our own standpoint, the pertinence of which the reader, having the above quoted sentence before him, will have no difficulty in determining:

"Permit me," Professor Loew went on, "the following remarks regarding this remarkable sentence" (the one quoted above):

1. It is not a *theory* that magnesium salts act poisonously on plants; it is a *fact*.

Miss Burlingham did not say it is a "*theory* that magnesium salts act poisonously on plants"; she herself witnessed such poisonous action repeatedly, and wrote as follows about this very "*fact*" in her abstract, although Professor Loew has not seen fit to quote it: "Magnesium sulphate . . . is usually toxic in strengths greater than $m/8,192$ (0.003 per cent.)"; anhydrous, 0.00147 per cent.

2. Not only Loew, but also others have observed the same fact. Loew has merely furnished an explanation well in accord with certain observations.

Miss Burlingham did not intimate that Professor Loew was the only investigator who had "observed the same fact," *i. e.*, that "magnesium salts act poisonously on plants." She knew quite well there were others, among them herself, as is indicated in the above quotation from her abstract that Professor Loew failed to notice. She did not allude to Professor Loew's "explanation that is well in accord with certain observations." She referred, however, in the words indicated, to "Loew's theory that magnesium salts when alone in solution are *always* injurious to plant growth." Professor Loew did not discuss the latter point in his letter, however, although he might well have done so to the exclusion of the matters he introduced without warrant. Why did he refrain from correcting the essential point in his quotation? *Are* "magnesium salts when alone in solution *always* injurious to plant growth"?

3. The doses at which magnesium salts, applied alone, are poisonous for plants can *impossibly* be called *excessive*, since even at 0.02 per cent. a poisonous action of magnesium salts on algæ can be observed, while calcium nitrate is not in the least injurious for algæ at even 1 per cent.

"*Excessive*" is, of course, a relative term and Miss Burlingham used it as such. In the abstract from which Professor Loew quoted the "remarkable sentence" which, according to him, "*requires* rectification in the interest of the unprejudiced reader," but which he proceeded aggressively to misconstrue, regardless of what the "interest of the unprejudiced reader *required*," Miss Burlingham wrote as follows: "It was found that while magnesium sulphate is usually toxic in strengths greater than $m/8,192$ (0.003 per cent.), it produces decided stimulation in $m/16,384$, reaches a maximum stimulation at dilutions from $m/32,768$ to $m/131,072$ (0.00075 per cent. to 0.00018 per cent.), then beyond this point gradually diminishes in action. . . . Seedlings allowed to grow for several weeks in a dilution of magnesium sulphate which was at first slightly toxic finally developed strong lateral roots and attained a root growth far beyond the control." It is obvious that Miss Burlingham used the term "excessive" to apply to "strengths greater than $m/8,192$

(0.003 per cent.)," *i. e.*, of magnesium sulphate (anhydrous, 0.00147 per cent.).

4. It is a well-known fact that many compounds that act poisonously at a certain concentration can act in very high dilution as stimulants of growth.

Miss Burlingham said nothing to the contrary. She found nothing in opposition to it. There is nothing in her abstract to warrant the inference that she was not aware of this "well-known fact."

5. It is erroneous to attribute this stimulating action to any nutritive quality of the poison.

Miss Burlingham did not "attribute this stimulating action to any *nutritive* quality of the poison." She said her results "show conclusively that magnesium sulphate in *proper* dilution is *beneficial* to the growth of seedlings." She did not offer any explanation of her preliminary results, merely stated them.

It is ridiculous for Professor Loew to assume that Miss Burlingham exhibited prejudice in her abstract, for neither she nor I had any preconceived notions to establish, nor any theories to maintain. Her conclusions were drawn impartially from her results.

Professor Loew concluded his letter with the following unbiased allusion:

6. The unprejudiced reader who desires some information as to the nutritive rôle of magnesium salts in plants and to the conditions under which this function can be performed, is kindly requested to consult Bulletin No. 45 of the Bureau of Plant Industry, "The Physiological Rôle of Mineral Nutrients in Plants," Washington, 1903.

I cheerfully commend "Bulletin No. 45," of which Professor Loew is the author, to the attention of any one wishing "information as to the nutritive rôle of magnesium salts in plants and to the conditions under which this function can be performed." The said bulletin is the most valuable single contribution to our knowledge of the questions discussed in it, and reflects brightly the flood of light that Professor Loew has thrown upon the subject since he undertook its investigation. Nevertheless the "unprejudiced reader" of it will certainly conclude, after studying "Bulletin No. 45," that there is probably very much more for all of us, including Professor Loew, to learn about the "nutritive rôle of magnesium salts in plants" and "on the conditions under which this function can be performed." The "unprejudiced reader" will also surely welcome such earnest attempts as Miss Burlingham's to extend our information on details of the subject.

Miss Burlingham's preliminary paper appeared in the July number of the Journal of the American Chemical Society. It gives the data upon which were based the remarks in her abstract that Professor Loew has misinterpreted for the "unprejudiced reader." It makes further comment here unnecessary.

William J. Gies
New York Botanical Garden

A NOTE ON CERTAIN WIDELY DISTRIBUTED LEAFHOPPERS (HEMIPTERA)

Certain leafhoppers have more or less recently become notorious for the damage they occasion to various cereals, such as sugar-cane and sorghum. *Perkinsiella saccharicida* (Kirkaldy) has done much damage in Hawaii, having been introduced from Queensland, where, however, it is not native. It is to be found wherever sugar-cane is grown in Australia and Hawaii, and I have recently received it from Java. *Peregrinus maidis* (Ashmead) was described from maize in Florida and is now widely distributed over the southern United States; it has an even wider range now than *Perkinsiella saccharicida,* for it is all through eastern Australia, Hawaii, Viti and, I think, Java, while Mr. Distant has recently redescribed it as *Pundaluoya simplicia* from Ceylon.

G. W. Kirkaldy

SPECIAL ARTICLES

COLOR INHERITANCE AND SEX INHERITANCE IN CERTAIN APHIDS

The color changes that occur in the sexual generation of certain aphids, and the correlation of a definite color with each sex, have suggested that these insects may furnish favorable material for testing the possibility

that the male and female sex characters form an allelomorphic pair and undergo segregation in gametogenesis.

In one of the goldenrod aphids all of the parthenogenetic individuals are a deep reddish brown, the males are green and the females brown, both males and females being produced by the same mother. Assuming that sex may be regarded as an inheritable character, the indications are that the parthenogenetic individual is both a sex-hybrid and a color-hybrid, green color and male sex being recessive. In the sexual generation green color becomes dominant with the male sex, brown with the female. Here correlation of color with sex, and selective fertilization, *i. e.*, only gametes containing opposite sex characters forming fertile unions, would account for the conditions observed.

In another aphid found on the star cucumber, the parthenogenetic generations consist of green and red individuals. Both red males and green females are produced by the same parthenogenetic mother which may be either green or red. Here again it appears that all of the parthenogenetic individuals must be both sex-hybrids and color-hybrids, but either color may be dominant during the parthenogenetic generations with no evident determining factor.

In an aphid which is abundant on the flower clusters and upper leaves of *Œnothera biennis*, we find more complicated conditions. In the parthenogenetic generations there are two colors, a dark red and a bright green. In autumn certain red-winged mothers produce red apterous females, and other apterous red individuals produce greenish-brown males, while red females and green males come from the green mothers. The winged males are produced only by apterous mothers, the apterous females only by winged mothers. In this case all may or may not be sex-hybrids and color-hybrids.

In November, 1905, I placed sexual forms of these *Œnothera* aphids on *Œnothera* rosettes in the greenhouse and an abundance of eggs were laid. The eggs hatched early in March, giving both red and green young. Individuals of the two colors were isolated on *Œnothera* plants protected with fine tarletan, and the several families were kept under observation in the greenhouse until June 14, about three months from hatching. All of the members of each family remained true to the color of their egg-ancestor. The plants with the aphids were then taken to Cold Spring Harbor and planted out under tarletan screens. Syrphid larvæ killed many of the aphids. The last of the green ones disappeared in August, while some of the red ones lived until the last of September. In no case did any individual of any one of the families deviate in color from its egg-ancestor. Sexual forms did not appear before I left Cold Spring Harbor on September 28. After returning to Bryn Mawr, about October 1, I collected both green and red parthenogenetic aphids from wild *Œnotheras* and raised the males and females of the sexual generation from these. The males of green parentage are bright green like their mothers, while the females are pale green when born and gradually grow more and more reddish until, when mature, they are a bright red, not quite so deep a red, however, as that of the red females from red mothers. The males of red parentage are red when born, but change gradually to a greenish brown, while the females are deep red like the mothers, the red being a little brighter at maturity than that of the parthenogenetic generations, but easily distinguishable from the brighter and more transparent red of the females of green parentage.

It will thus be seen that the color which comes from the winter egg holds for all of the parthenogenetic descendants, but when the sexual forms appear the males are green or greenish brown, and the females red, indicating some relation between color dominance and sex. That this relation can not be associated with different metabolic conditions in the two sexes is shown by the fact that in the star cucumber aphid, where there are both green and red parthenogenetic strains, the color conditions in the sexual generation are reversed,—the males are red and the females green.

A few preliminary experiments were made in November, 1906, in mating males and

females raised from isolated mothers in the greenhouse. The matings and results were as follows, the letters showing the color characters which were visible:

1. ♀ R (red par.) × ♂ RG (red par.) { *a.* All red. *b.* Red and green.
2. ♀ GR (green par.) × ♂ G (green par.) { All green.
3. ♀ R (red par.) × ♂ G (green par.) { No eggs hatched.
4. ♀ GR (green par.) × ♂ RG (red par) { *a.* One red. *b.* Several green.

Only a small proportion of the eggs hatched, but the results, though meager, indicate the possibility that color inheritance may here be Mendelian, and that a further study of it may throw light on the problem of sex inheritance.

The coloration of the sexual generation, however, shows that either one or the other of two conditions must probably exist: (1) All of the egg-ancestors and therefore all of the parthenogenetic individuals, as well as the males and females, are sex-hybrids as well as color-hybrids, and the factors which determine sex dominance also determine color dominance, possibly by virtue of some structural correlation of the two characters. (2) There are green hybrid strains which produce red females and red hybrid strains which produce greenish-brown males, while the red strains which produce red females may be pure reds and the green strains which produce green males may be pure greens.

The first of these suppositions, which alone could account for the conditions found in the star cucumber aphid, where parthenogenetic mothers of either color produce both red males and green females, and in the goldenrod aphid where the brown parthenogenetic mothers produce both green males and brown females, seems much more likely to be true for all.

In the following table the possibilities for the star cucumber aphid are shown under I., and those for the *Œnothera* aphid under I. and II. combined. (The color scheme must be reversed for the star cucumber aphid, G♀, R♂.) In both, the dominance of sometimes one color, sometimes the other in the parthenogenetic generations is a subject for investigation. It may be conditioned by the immediate ancestry of the gametes.

	Gametes	Parth. Gen.	Sexual Gen.	Gametes
I	G ♂ / R ♀	G(R) or R(G)	♂ G(R)	G ♂ sp. / R ♀ sp.
			♀ R(G)	R ♀ egg / G ♂ egg
II	G ♂ / G ♂	G	♂ G	G ♂ sp.(?)
	R ♀ / R ♀	R	♀ R	R ♀ egg

In the goldenrod aphid, if we consider the parthenogenetic forms as essentially female, correlation of color with sex (B♀ and G♂), and selective fertilization would account for the observed relation of color to sex.

The second, and less likely but nevertheless interesting, possibility for the *Œnothera* aphid involves the question whether a zygote can be pure as to the sex character, or unisexual. The chief point to be investigated by experiment, in addition to the study of color inheritance in cross-breeding, is whether in this aphid both males and females come from the parthenogenetic progeny of each egg-ancestor, or in some cases (G♂) only males, and in others (R♀) only females. To test this possibility it would be necessary to carry many families through from the egg to the following sexual generation, and very likely to repeat the experiment several times.

A large series of experiments in cross-breeding to test the color inheritance has been planned by the author for next year, and this note is published in the hope that some one may be interested to undertake experiments along the same line.

N. M. Stevens
Bryn Mawr College,
Bryn Mawr, Pa.,
June 8, 1907

A COLOR SPORT AMONG THE LOCUSTIDÆ

There are various sports among animals that are so rarely observed and so little understood as to seem to render it desirable that every occurrence should be recorded. One of these is the occasional substitution of pink for green color among the Locustidæ, which has been recorded perhaps a dozen times. It is to be hoped that repeated notices of their capture may call the attention of physiologists to them, and in time elicit a satisfactory explanation of the phenomenon. A specimen of

one of these pink katydids, a male of the species *Amblycorypha oblongifolia* (De Geer), identified by Jas. A. G. Rehn, was sent to the Museum of the University of Michigan some months ago, by Mr. A. S. Austin. He captured the insect on Grosse Isle, in the Detroit River, some twelve or fifteen miles below Detroit, on August 12, 1906. This specimen is of duller colors than the ones figured by Scudder,[1] but is still a decided pink. The brown spots on the tegmina are fainter, twelve or thirteen in number, and less scattered than in Scudder's specimens, and are roughly arranged in two rows on the lower two thirds of the wings. The yellow flecks mentioned by Scudder are wanting.

The stridulating area, like that of Scudder's specimen, is brown except a small trapeziform area at the angle of the wing, which is pink. The eyes are brownish red, margined with yellow. The antennæ are yellow, inclined to brown in the distal half. The sides of the thorax, all of the coxæ, and the proximal fourth of the hind femora are tinged with green.

Besides this specimen there is also in the University Museum a female of the same species, without data, which has also duller colors than the female of Scudder's plate. This dullness can hardly be attributed to a fading in the preserved specimen, for the male was received alive, and up to the present time has preserved its colors perfectly.

Folsom[2] has been led to remark that these pink specimens are found in late summer, as if to suggest that the change may be at least in part due to seasonal influence. If this statement is meant to apply to adults only, the earlier dates, August 9 for one of Scudder's specimens and August 12 for Austin's, can hardly be considered late records. If the replacement occurs also in nymphs, of which I find no record in the accessible literature, then the earliest records are comparatively late, and Folsom's suggestion is of some force. It would be gratifying to learn the dates of other unrecorded specimens.

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[1] Scudder, S. H., 'Pink Grasshoppers,' *Entomological News*, Vol. XII., No. 5, May, 1901, pp. 129–131, and Pl. VI.

[2] Folsom, J. W., 'Entomology with Reference to its Biological and Economic Aspects,' p. 215.

GYMNOSPORANGIUM MACROPUS

During the last few years the cedar rust has increased in abundance and severity in Nebraska and adjacent states. The disease has been so severe that it has threatened the life of the cedars in many places where they have been employed as wind-breaks around orchards or where they were adjacent to apple trees that were susceptible to infection.

Spraying experiments have shown that the disease may be at least partially controlled on the apple, but those who have cedars—and there are many in this section who value them as much as they do their apple trees—have been clamoring for assistance in saving them from the inroads of this fungus.

As a result of this demand careful observations have been made on the life history of this rust and spraying experiments are in progress. The work was started with the supposition that the spores of the cluster-cups on the apple leaves and fruit produced the cedar apples which matured in the autumn of the same season, but observations and experiments have not confirmed this assumption.

Observations made during the summer of 1906 showed that the first cluster-cups matured on the apple about the first of July in the vicinity of Lincoln. At this date only a very few cluster-cups were open and these were mostly on the fruit where two or more apples were in contact. A few days later young cedar apples as large as radish seeds were found to be present on the cedars. At Broken Bow, Custer County, the first mature cluster-cups were not observed until the eighth of July, and at that time cedar apples were found in abundance varying in size from one twelfth to one fourth inch in diameter.

Careful watch has been kept of the cedars during the present season. The first indication of the presence of the young cedar apples

was noted on the seventeenth of June. At this time the scale leaves at the points of origin of the apples were slightly lifted. On June 26, the young apples had increased in size so that they could be easily detected with the hand lens or even with the naked eye. All apple trees and other possible hosts were carefully examined and the rust spots showed at that date nothing but the spermogonia. They did not even show the characteristic hypertrophy of the under surface which precedes the formation of the cluster-cups, and the stage of development at present indicates that no mature æcidiospores will be formed until near the middle of July. This apparent retardation of the development over that of last year is to be explained by the general backwardness of vegetation due to the cold spring.

In addition to these observations I should mention that some small cedars were enclosed in glass houses during the spring of 1906. These houses were ventilated by means of windows provided with cotton screens to prevent infection from the outside. The first part of July they were examined and a few cedar apples were found, the small number being due presumably to the fact that conditions in the houses were very unfavorable for growth.

Considering these observations here recorded, two explanations suggest themselves:

1. The fungus is either perennial in the cedar, or

2. The æcidiospores of one season produce the cedar apples which appear in June of the next year and reach maturity in the autumn.

We have some evidence of a perennial character, especially in trees that are badly infected. In such cases it is quite easy to find new apples growing out from the side of old ones, or even from the middle of old ones. It is, however, quite possible that such cases represent new infections rather than the persistence of an old mycelium. The second explanation however seems more probable to the writer. If this is true the cedar is probably infected in the summer and autumn, but no evidence of the resulting cedar apples can be noted until the next season when growth has been resumed. It would then require two full years for a cedar apple to develop. It remains for further observations to completely substantiate this view.

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A BLIGHT DISEASE OF YOUNG CONIFERS[1]

During the past spring there occurred in the large conifer nursery at Halsey, Nebraska, a very serious outbreak of "blight" of the needles of two-year-old seedlings of *Pinus ponderosa* and *P. divaricata*. The damage was very considerable, there being several hundred thousand of the trees affected. What is of more moment than the actual damage sustained, however, is the threatened danger to the many nurseries of the country which are engaged wholly or in part in the growing of young conifers for reforesting purposes. The present outbreak shows that the fungus causing it is capable of very serious and extensive attacks wherever it may happen to be present. The disease is characterized by a gradual dying back of the needles from the tip to the base. The fungus very evidently then proceeds into the stem of the affected tree and finally kills the entire plant. In the specimens of diseased trees examined by the writer no fungous fruiting bodies could at first be detected; upon remaining in a moist chamber for a few days abundant black pustules broke out upon all of the dead tissues of the attacked needles. These were found to be exuding masses of spores of a species of *Pestalozzia*. The pustules occurred universally upon all dead parts and no other organism thus accompanied the disease; it seemed apparent at once that the *Pestalozzia* was closely connected with the trouble. Pure cultures of the fungus were made and then used in making inoculations upon healthy seedlings of *Pinus ponderosa* in the greenhouse, which were about one month old. The inoculations succeeded, causing the typical disease in plants which

[1] Published with permission of the Secretary of Agriculture.

had been previously healthy in every respect.

The various species of *Pestalozzia* have been known as parasites in Europe, causing disease of conifers of one to several years' age. The same fungus was found in 1903 in Texas and also upon young pine trees from North Carolina and New York in 1906 by the writer. There can be little doubt that it occurs generally in the United States, and sometimes at least, as a true parasite. The fact that it occurs as a parasite upon young conifers seems not to have been proved in this country by other workers. The present article may be taken as a warning to managers of conifer nurseries, as it is more than likely that similar outbreaks of this disease will be noted in the near future. Removing the diseased trees and burning them, accompanied by thorough spraying of the remainder with Bordeaux mixture containing some adhesive substance to make it cover the smooth needles, should completely control the trouble and stop its spread into unaffected seed beds.

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NORMAL FAULTING IN THE BULLFROG DISTRICT

The Bullfrog Mining District is situated in southern Nevada, about ten miles from the California line, and sixty miles south-southeast of Goldfield. The towns within the district are Rhyolite, Beatty and Bullfrog. In 1906 Mr. G. H. Garrey and the speaker mapped the general geology of a strip across the district, seven miles long and three miles wide. The country is a desert, the rocks are bare and exposures are exceptionally good. The relief is about 2,500 feet. Mining exploration has added greatly to the natural exposures, and conditions for field work are unusual.

The oldest rocks form a crystalline complex, consisting in the main of quartz-biotite schists, quartzites, limestone, pegmatite, injection schists and gneisses, which surround small areas of sheared diorites. This complex is the equivalent of a series of sedimentary rocks which has been greatly metamorphosed. Above the schists is a massive limestone, about 100 feet thick, probably Silurian. In faulted contact with the limestone and older rocks is a great series of Tertiary lava flows with subordinate beds of sedimentary tuffs, limestone and shale, altogether about 7,000 feet thick. Of the lava flows there are sixteen separable divisions of rhyolite, five basalt flows, one flow of dacite, and one of quartz basalt. Stratified tuffs of sedimentary origin occur at two horizons, with numerous lava flows between. The Tertiary rocks are approximately conformable one with another in dip, though there are slight erosional unconformities at several places. Basalt dikes, most of which are along fault fissures, cut the older lavas. There was much faulting after the dikes were intruded, and the rhyolite-basalt contacts afforded planes of weakness which were taken advantage of in nearly every instance. Dikes and other intrusive masses of rhyolite also cut the lavas. At three places there are small outcrops of leucite basanite.

The bedded rocks dip eastward at angles averaging 27° and are traversed by faults, most of which strike northeast and dip west. Most of the faults are nearly perpendicular to the beds and all are normal, that is, the down-throw appears to have been down the dip of fault planes and consequently the west block is, in most cases, depressed, or the block east of the fault plane is elevated with respect to the down-thrown or hanging-wall side. Since the dip of the beds is to the east and the dip of the faults to the west, the same beds occur repeatedly. Before deformation the beds were approximately horizontal. In the deformation two processes operated; faulting, which tended to lower the beds to the west or raise them to the east; and monoclinal folding or tilting, which tended to raise the beds towards the west or lower them to the east. A seven-mile east and west section across the area shows that the eastward depression due to tilting is 12,400 feet, which is only 1,300 feet more than the westward depression due to faulting, or that the result of both processes was to leave the beds at about the same elevation at the east and at

the west borders of the area. The throw of the faults varies from a few feet to 5,000 feet. There are two systems or groups, one of which strikes nearly north and the other about 35° east of north. The Tertiary rocks are not closely folded, but the dip of the beds in any single block is nearly uniform.

It is improbable that any considerable amount of tilting or faulting occurred before all of the Tertiary lavas were extravasated, for the dip of early and of late flows is nearly uniform, and lavas do not overlap faults. The tilting occurred before or after faulting, or else the two processes went on together. If all of the tilting had occurred before the faulting then a given bed at the east border of the area should at that time have been 12,400 feet lower than the same bed at the west border. Evidence of such relief should be preserved if the period between the deformation by the two processes had been sufficient for a considerable amount of erosion, and a large thickness of derived sedimentary rocks should probably have resulted from the erosion of this series. On the other hand, if the faulting had occurred first and the interval was considerable, the relief and consequent intervening erosion would have been equally great. Since there are no faulted rocks not tilted, or tilted rocks not faulted, it is presumed that faulting and tilting operated at the same time or close together.

Tilting before faulting implies a vertical movement of parts of the earth's surface of more than two miles, followed by another vertical movement equally great and of a different character. Faulting before tilting implies equivalent movements in reverse order. Since the processes operated close together, this is regarded as improbable. It is, therefore, assumed that faulting and tilting occurred at the same time, and that the movement was largely rotational, each block moving independently, being tilted as it was faulted. The result is like the fall of a row of books when some are removed from the shelf. It is to be noted that when the books fall and become inclined 27° from an upright position, there is an extension of a line drawn horizontally through them equal to 12 per cent.; that is, some books must be removed if the remainder fall. Unless there was extension due to revolution some of the blocks must move out laterally in order that the other blocks may settle. The faults are not quite parallel in strike, but two systems make 35° angles with each other. Accordingly, some of the blocks would present wedge-shaped edges to any section and these during deformation could easily move laterally outward. That lateral movement did take place is abundantly recorded by nearly flat striæ on horizontal surfaces. The effect of all deformation was to greatly extend the surface east and west in the direction of the dip of the beds.

W. H. EMMONS
U. S. GEOLOGICAL SURVEY

QUOTATIONS

THE NOBEL PRIZES

REGRET has already been expressed here that the confidence placed by Nobel in his native land has not been justified. His large fortune was made in Great Britain by the discovery and manufacture of dynamite, and it seems likely that the instructions of his will would have been more adequately carried out if their execution had been entrusted to the Royal Society and the British courts. Nobel doubtless believed that the international obligations would be fully met by the Scandinavian countries, and it is truly sad and discouraging that there should be lack of good faith in the administration of a fund intended, as the testator states, "to benefit mankind."

Nobel's will is perfectly clear and explicit. It directs that the interest from the fund "shall be divided into five equal parts," which shall be annually awarded in prizes to those persons who shall have contributed most materially to benefit mankind during the year immediately preceding. "One share to the person who shall have made the most important discovery or invention in the domain of physics; one share to the person who shall have made the most important chemical discovery or improvement; one share to the person who shall have made the most important discovery in the domain of physiology or medicine; one share to the person who shall

have produced in the field of literature the most distinguished work of an idealistic tendency, and, finally, one share to the person who shall have most or best promoted the fraternity of nations and the abolishment or diminution of standing armies and the formation and increase of peace congresses."

In face of these explicit directions statutes have been drawn up, apparently with the sanction of the King of Sweden and others high in authority, providing that only sixty per cent. of the income need be used for the prizes and that they need be awarded only once in five years. The balance of the income—except perhaps in the case of the prize for the promotion of peace, regarding which information is lacking—is now used for the support of certain laboratories and libraries at Stockholm. These are doubtless needed, possibly more than the prizes established by Nobel, but they have been founded in dishonor. The clause establishing the laboratory of physics and chemistry is unpleasantly disingenuous. It says that it is to be "established primarily for the purpose of carrying out, where the respective Nobel committees shall deem requisite, scientific investigation as to the value of those discoveries in the domains of physics and chemistry which shall have been proposed as meriting the award of Nobel prize to their authors. The institute shall, moreover, as far as its means allow, promote such researches in the domains of the sciences named as promise to result in salient advantage." The prizes have so far been awarded annually, but it is to be feared that when the money is needed in Sweden, it will be kept there in accordance with the provision of the statutes that when a prize is not awarded the money may be used for funds "to promote the objects which the testator ultimately had in view in making his bequest in other ways than by means of prizes."

The administrators of the Nobel foundation have violated the conditions of the bequest in other ways which, though not so discreditable as the conveying of the money to local purposes and men, can not be regarded as justifiable. Nobel expressly stipulates that the prizes shall be awarded to those "who shall have contributed most materially to benefit mankind during the year immediately preceding." The statutes hedge, as follows: "By the proviso in the will to the effect that for the prize competition only such works or inventions shall be eligible as have appeared "during the preceding year" is to be understood that a work or invention for which a reward under the terms of the will is contemplated shall set forth the most modern results of work being done in that of the departments, as defined in the will, to which it belongs; works or inventions of older standing to be taken into consideration only in case their importance has not previously been demonstrated."

In no single case has the award been made for work accomplished or published during the preceding year. The prizes have been given to men of eminence, most of whom accomplished their important work long ago. It would certainly be difficult to select each year the work most beneficial to mankind, and mistakes would undoubtedly be made; but the effort to make such a selection and to award the prize without regard to nationality, age or eminence would be a great stimulus to research, far greater probably than the methods adopted. But the question is not which method is the better, but for what purposes Nobel made his bequest. The terms of the will have also been violated by dividing the prizes and by awarding them to institutions, and its spirit has been especially ignored by giving the power of nomination and determination chiefly to Swedes. It does not of course follow that the dead hand should forever control. But Nobel died only ten years ago. He might be given his will for a little while at least, and under the special circumstances of the case it would seem only just to submit any provisions which proved impracticable or unwise to international consideration.

There is a certain lack of courtesy in thus criticizing actions sanctioned by the Swedish government and by those Swedish men of science at least who are accepting gratuities from the fund. Neither can we as a nation regard ourselves as fit to cast stones when we

remember the histories of the Stewart, Tilden and other bequests, or when we consider that the Smithsonian Institution, established by a foreigner "for the increase and diffusion of knowledge among men" has been used largely for the promotion of local interests. But it is only by frankly considering these things that we may learn that honor is more than great riches.—*The Popular Science Monthly*, January, 1907.

We agree with Mr. Lange when he says, on page 1060 of this issue, that Dr. Alfred Nobel was a man of remarkable originality, as is shown by his bequest of his fortune to Scandinavia to reward the benefactors of mankind. But we fear that his originality will never be allowed much scope by those who have charge of the administration of the fund, for they have from the beginning shown a flagrant disregard of the intentions of the founder. This, of course, is no new thing. Many philanthropic testators, if they could rise from their graves fifty or a hundred years after they had been laid in them, would repudiate the work that is being carried on in their names. This is sometimes the fault of the trustees and sometimes their wisdom. The provisions of a will may prove to be impracticable, or in the course of time the changed conditions may make it useless or detrimental to the cause it was intended to promote.

But Nobel's plan has been proved neither unpractical nor unwise, because it has never been tried. In his will of November 27, 1895, he directs that his property "shall constitute a fund, the interest accruing from which shall be annually awarded in prizes to those persons who shall have contributed most materially to benefit mankind *during the year immediately preceding.*"

The clause we have italicized has been disregarded from the start by the five Nobel committees, although it is the most original and promising feature of the plan. Great discoveries in science and innovations in literature are often the work of young men, unappreciated by their colleagues and superiors, overburdened by drudgery and inadequately provided with the means of study and research. To men like this the free gift of $40,000 and the public recognition of the value of their work would be a godsend. They would be stimulated to greater exertions and would be able to devote themselves to the work for which they had already proved themselves exceptionally fitted.

But the Nobel committees, instead of this, have chosen to bestow their awards in many cases on men who, long before the Nobel Fund was established, had done the work for which the world is their debtor, and were resting on their laurels. The money, however much needed, will not enable them to do more than they have; the honor, however much deserved, will not add to their fame. The Nobel prizes have been given only six years, yet six of the recipients—Carducci, Moissan, Dunant, Mommsen, Finsen, Curie—have died since they were so honored, three of them from old age. The following table shows how far the Nobel committees have departed from the intention of the founder in rewarding contemporary achievement:

Name	Achievement	Age of Achievement	Age of Award	Years of Delay
Dunant	Geneva Convention	36	73	37
Sully-Prudhomme	"Justice"	39	62	23
Mommsen	"History of Rome"	37	85	48
Fischer	Sugar synthesis	33	50	17
Björnson	"Arne"	26	71	45
Mistral	"Mirêio"	29	74	45
Echegaray	"O Locura ô Santidad"	45	71	26
Passy	French Arbitration Society	45	79	34
Arrhenius	Electrolytic theory	25	44	19
Becquerel	Uranium rays	44	51	7
Behring	Diphtheria antitoxin	38	47	9
Ramsay	Helium	43	52	9
Finsen	Light cure	34	41	7
Cremer	Interparliamentary conference	50	65	15
Rayleigh	Argon	52	62	10
M Curie	Radium	39	44	5
Madame Curie	Radium	31	36	5
Röntgen	X-rays	50	56	6
Ross	Malaria parasite	40	45	5
Carducci	"Odi Barbare"	44	71	27
Ramon y Cajal	Neurology	41	56	15
Moissan	Isolation of fluorine	35	44	9
Baeyer	Artificial indigo	45	70	25
Koch	Tuberculosis bacillus	41	64	23
Sienkiewicz	"With Fire and Sword"	38	59	21
Lenard	Lenard rays	32	43	11
Suttner	"Die Waffen nieder"	47	63	16
Golgi	Nerve staining	25	58	33

The Code of Statutes of the Nobel Foundation, issued in the name of the King, June 29, 1900, contains the following section:

The proviso in the Will to the effect that for the prize competition only such works or inventions shall be eligible as have appeared "during the preceding year" is to be so understood, that a work or an invention for which a reward under the terms of the Will is contemplated, shall set forth the most modern results of work being done in that of the departments, as defined in the Will, to which it belongs; works or inventions of older standing to be taken into consideration only in case their importance have not been previously demonstrated.

This action loosened up the stringency of the phrase used by Nobel, but the committees have not even kept within the elastic limits that they imposed upon themselves, as a glance at the table shows. What we have put down as the "age of achievement" is the year of the man's life when he produced his first work of superlative importance, the excellence of which was either recognized at once by the world or would have been discernible by a learned and well-equipped body like the Nobel Committee. But in many cases, nothing had occurred to "demonstrate the importance" of their achievements during "the preceding year," or even during the time the Nobel Foundation has been in existence. Carducci was too weak to rise from his chair when the emissaries of the Nobel Committee brought him his medal and too feeble in mind to answer them. He had not published a book for nine years, and his position as the foremost of Italian poets had been established for over thirty years. The fame of Sully-Prudhomme, Echegaray and Mistral has declined rather than risen in the last six years, because they have become more historic monuments than leaders of modern thought.

Mr. Lange defends the appropriation of 25 per cent. of the income for administrative expenses on the ground that it is necessary in order to insure that the prizes are worthily bestowed. This might be justifiable if the money were spent for this purpose. If the committees used the laboratories and libraries they have established out of the Nobel Fund for the purpose of testing the real value of alleged inventions it would do much to promote science and assist in the discovery of struggling genius. But no man is allowed to present his own claims. He must first have the endorsement of scholars occupying certain narrowly specified official positions in his own land.

As a matter of fact, the selections of the Nobel Committees have not been such as required special ability or expenditure for investigation. Any college student in chemistry, physics or medicine, if asked offhand to name the greatest living men in his branch of science would have hit upon at least fifteen out of the twenty-two names on the list of the Nobel prize men. In the choice of those who had done most for the promotion of peace or produced the greatest work in idealistic literature there would have been greater diversity of opinion, but not because the names chosen were not well known. Did it require an $80,000 laboratory to test the reality of the X-rays? How much of the "rather more than $12,000" appropriated for that purpose last year did the committee expend in repeating Baeyer's synthesis of indigo, first made a quarter of a century ago, and now accomplished at the rate of thousands of tons a year? Did the Caroline Medical-Chirurgical Institute of Stockholm have to spend much time in ascertaining that Golgi's method of nerve staining, which has been in common use for over twenty years, is practical and valuable? How large a reference library was needed to discover that Mommsen was a great historian?

The Nobel bequest was reported to be more than $8,000,000. This, if invested in safe securities, as Nobel directed, should produce about $64,000 for each of the five annual prizes. So much of the income has been spent for other purposes, in salaries, traveling expenses, ceremonials and purchases of books and apparatus, that the amount of the money prize has now shrunk to $37,000. And still the local administrators are not satisfied with what they get out of it. Mr. Lange suggests that they may take advantage of the clause allowing them to suspend the award for not longer than four years in the absence of suitable candidates in order to get money for the "constructive" work of the Nobel Peace Institute, for the maintenance of a library and

reading-room in Christiania, for a complete catalogue of the literature of internationalism, a school, the printing of books and periodicals and the establishment of another arbitration court. These are highly creditable projects, but the Nobel Fund was given for another purpose. All the countries of the world have the same interest in it as Norway and Sweden, and they have a right to protest against its misappropriation.—*The Independent,* May 9, 1907.

CURRENT NOTES ON LAND FORMS

PIT CRATERS IN MEXICO

Among the many basins of the Central Plateau of Mexico, bordered by volcanoes in various stages of growth and dissection, and smoothly floored with aggraded layers of volcanic ashes and dust, of fluviatile and lacustrine desposits, and of occasional lava flows, there is one of typical development in the state of Puebla, east of the city of Mexico and separated by the volcano of Orizaba and its neighbors from the dissected escarpment by which the descent is made from the highland to the coastal lowlands. Ordoñez gives a good account of this basin-plain and of the pit craters that have been formed in it by explosion ("Los Xalapascos del Estado de Puebla," Inst. Geol. Mex., Parerg., i, 1906, no. 9). The plain is like all its fellows in having risen on the irregular flanks of the larger and smaller, younger and older volcanic masses that enclose it, and in being interrupted by more or less completely isolated volcanic knobs and ridges which rise here and there through its smooth surface. The gentle ascent by which one ordinarily approaches the border of an explosion crater is an insignificant element of relief in comparison with the much larger volcanic forms on all sides; indeed, the slope is sometimes hardly perceptible, and the depression of the crater, 1,000 to 1,800 met. in diameter, and 50 or more met. deep, is come upon as a surprise for which there is no warning at a little distance. A shallow blue lake usually occupies the floor; the walls are frequently steep and expose good sections of the layers by which the plain has been built up; special interest attaching to such items as buried stream channels and occasional thin lava sheets. Paths lead down in zigzags on the steep face or more directly by centripetal ravines; for the poor natives in neighboring villages have long been accustomed to carry water up from the lakes for domestic uses. Ordoñez regards these craters as among the latest manifestations of volcanic activity, and characterizes them as seeming unduly large for the feebleness of the explosive force which produced them.

BATOKA GORGE OF THE ZAMBESI

Among the results of the British Association visit to South Africa in 1907 is an account of "The Geology of the Zambesi Basin around the Batoka Gorge," by G. W. Lamplugh (*Quart. Journ. Geol. Soc.,* LXIII., 1907, 162–316), which includes an excellent description of a plateau in a youthful stage of dissection. In the region of Victoria Falls, the South African highland is built up of basalt sheets; older rocks, including a fundamental complex of gneiss, schists and granite, appear to the northeast and southeast. The relief is small; occasional residual knobs—*Inselberge* of the German explorers—rise here and there in the crystalline areas; low escarpments traverse the belts of inclined strata bordering the crystallines; broad swells of sand, supposed to be wind deposits of an earlier and more arid period, are spread over the basalts. The present altitude of the plateau is 3,000 feet or more. The upper Zambesi is a wide, placid river flowing through a shallow valley, bordered by low slopes of greatly decomposed basalt; its branches are of gentle fall and their valleys (called "channels" by Lamplugh) are but little below the general highland level. At Victoria Falls, the river plunges down 360 feet into a narrow gorge with nearly vertical walls, in which the peculiar zigzag turns have been well explained by Molyneaux ("The Physical History of Victoria Falls," *Geogr. Journ.,* XXV., 1905, 40–55) as the result of groups of obliquely transverse joints. In 60 miles below the falls, the river descends about 400 feet, and the walls of the gorge become more and more open. At the same time, the side gorges increase in length,

so that a widening area of sharply dissected country, very difficult to traverse, extends eastward from the falls, on each side of the main gorge. Some 300 miles east of the falls, the Kafue river, coming from the north, flows in a broad shallow valley for several hundred miles across the undulating plateau country, then plunges down through a succession of cataracts in a rugged gorge, descending 1,000 feet in two miles, after which it has a sluggish course of 20 miles to its confluence with the Zambesi.

The systematic manner in which the forms appropriate to old age on the plateau are replaced by those appropriate to youth in the gorge and its branches gives new warrant—if any new warrant is needed—for the use of a systematic terminology in the explanatory description of land forms. The structure of the region being stated in the first place, it suffices to say that the old features of the plateau are replaced by young features of strong relief below the falls; all the more characteristic forms may then be easily inferred. The space saved by the adoption of this concise style of description may then be used to advantage for accounts of individual features. For the highland area, "shallow valley," "shallow trough" and "channel sunk very slightly below the general level of the plateau" are the paraphrases which Lamplugh uses instead of the systematic term, "old valley"; the fact that three different descriptive phrases are thus used in a single article for one and the same class of forms only emphasizes the need of the adoption of a single, definite, technical term.

A PENEPLAIN IN SOUTH AFRICA

During the excursion of the British Association to South Africa in 1905, the undersigned had opportunity of traversing the High Veld of the interior on several different lines, and thus of gaining a general impression of its leading features (see "Observations in South Africa." *Bull. Geol. Soc. Amer.*, XVII., 1906, 377–450). As to structure, the region is broadly covered with a heavy series of nearly horizontal Mesozoic continental formations, resting unconformably on a complicated series of much older rocks, which appear to have been reduced in pre-Mesozoic time to the state of subdued mountains or hills, and which have since suffered still further reduction in the long continued cycle of erosion by which a vast body of material has been swept away. The present nearly-level highland of the treeless plains bevels across the Mesozoic strata at very gentle angles, and except for the scattered strong reliefs in the form of stony ridges and mesas maintained by resistant dolerite dikes and sheets, the surface has truly reached an expression of penultimate erosion. The low swells between the water courses have a thin soil; they are often of very faint convexity, nicely indicated by the faint arching of the long railway tangents. The water courses, unlike the broad and ill-defined wadies in the peneplain in equatorial Africa described in Science for August 2, 1907, are deep, well-defined channels, bordered by 20 or 30 feet of alluvium which cloaks the wide-open old-valley floors. The channels were nearly dry at the time of our visit, but bore the marks of having carried heavy floods in previous wet seasons (southern summer) when heavy local downpours occur. A curious minor item was the occurrence of rapids in the dwindled streams of the dry season, where rock sills occurred in the deep channel beds: this at first suggested a recent revival of erosion; but it was afterwards better understood that the long established grade of these old drainage lines is indicated by the even slope of their alluvial flood-levels and not by the small inequalities in their flood-scoured beds. Where the water courses lead through notches in the dolerite ridges, the channels are encumbered with boulders and their fall is more rapid than elsewhere; thus the Veld is divided into compartments of slightly different altitudes.

The Veld stands at altitudes of from 6,000 to 8,000 feet, with a gentle slope to the west, which turns a large drainage area to the Orange River system. The eastern border of the Veld is suffering invasion by the head ravines of actively retrogressive streams that descend rapidly to the coastal lowlands of the Indian ocean; and this feature, along with certain other indications, led to the belief that

the peneplain of the Veld had probably been worn down with reference to the normal base-level of the ocean when the region stood several thousand feet lower than now; and that its uplift is so recent that, over most of the surface, the long, west-flowing rivers have not yet had time to deepen their valleys in their upper and middle courses. Farther towards the Atlantic, it is to be expected that a beginning of incision must already have been made; but critical observations are lacking in that direction.

Further physiographic results of the same excursion are presented in an article on "The Mountains of Southernmost Africa" (*Bull. Amer. Geogr. Soc.*, XXXVIII., 1906, 593–623), where the heavy Mesozoic series and a conformably underlying Paleozoic series are folded in well-defined east-west anticlines and synclines, apparently peneplained in one cycle and greatly eroded in a second, with the result of developing a remarkably well-adjusted drainage system, containing excellent examples of subsequent and resequent streams, as well as of deep-cut transverse water gaps in the ridges. Many of the ridges are anticlines, and serve admirably to correct the prevailing misapprehension that the ridges of long-eroded mountains should be of synclinal structure.

W. M. D.

DEDICATION OF THE ALDROVANDI MUSEUM OF THE UNIVERSITY OF BOLOGNA, ITALY

With felicitous ceremonies, extending through June 11–13, the University of Bologna has dedicated to the memory of the illustrious seventeenth century Bolognese naturalist, Aldrovandus, a new geological museum. Amongst the foreign universities represented were Glasgow, Oxford, Cambridge, Berlin, Königsberg, Breslau, Halle, Vienna, Paris, Upsala, Christiania, Pennsylvania, Yale, Michigan, Cornell, etc.

The addresses on the principal day were delivered before a distinguished audience in the Archgymnasium, Senator Capellini, president of the University of Bologna, presiding. Following his eloquent address, a study of the *motif* of the occasion was given by Professor Costa. Responses from foreign countries were given by Professors Brusina, of Agram; Pélissier, of Montpellier; Ferguson, of Glasgow; Schück, of Upsala; Borcea, of Rumania; Richter, of Hungary, and Dr. Wieland, of the Carnegie Institution of Washington. The celebration was finally concluded by a dinner tendered the delegates by the mayor of Bologna.

The University of Bologna enjoys the proud distinction of being the oldest university in Europe, and possesses in addition to fine zoological collections, paleontological collections of great importance, as well remembered by Americans, due, largely, to the indefatigable efforts of Senator Capellini, now extending through a period of fifty years. This ancient university, so thoroughly imbued with the spirit of modern research and enterprise, is indeed to be congratulated on thus coupling the deep historical interest of the vast and wonderful pioneer labors of Aldrovandus, whom Capellini happily compares with Aristotle, with twentieth century science.

G. R. W.

CENTENARY OF THE GEOLOGICAL SOCIETY[1]

In September next the Geological Society will celebrate its hundredth birthday. In honor of this interesting occasion preparations have for some time been in progress. Invitations to the celebration have been issued to all the foreign members and foreign correspondents of the society; the various geological surveys all over the globe, universities having chairs of geology or mineralogy, scientific academies, societies and museums at home and abroad have been invited to send delegates to London. The large number of acceptances already received include the names of many of the most distinguished geologists of the present day, both in the old and the new world.

It has been arranged that a series of excursions to various parts of this country shall take place before the centennial meeting,

[1] From *Nature*.

under the conduct of fellows of the society conversant with the geology of the several selected districts. These excursions will begin on Wednesday, September 18, and the excursionists will all be back in London by the evening of September 25. The celebration of the centenary, which will extend over three days, will begin on Thursday, September 26, at 11 o'clock, in the Hall of the Institution of Civil Engineers, when the chair will be taken by Sir Archibald Geikie, who has been elected president of the society for the second time in order that he may preside on this occasion. The foreign members and foreign correspondents, and the delegates from institutions at home and abroad, will then be received by him, and will present their addresses. In the afternoon, at 3 o'clock, in the same hall, the president will deliver an address, while in the evening a banquet will be given by the society to its colonial and foreign guests.

Friday, September 27, will be chiefly devoted to visits to museums, galleries, etc., concluding with an evening reception. On Saturday, September 28, short excursions have been projected to places of geological interest within easy reach of London. On Monday, September 30, the visitors will be divided into two sections, one of which will go to Oxford, the other to Cambridge. It is understood that the universities will confer honorary degrees on some of the more distinguished geologists from beyond the seas, and that college hospitality will be as abundant and hearty as usual, while those visitors who may still have energy enough left for field-work will be taken on geological excursions from both the university towns. This well-planned combination of scientific intercourse with social pleasure can hardly fail to have a lasting effect in forming and confirming friendships by bringing the geologists of many different countries into close personal relations with each other.

SIR JOSEPH HOOKER'S NINETIETH BIRTHDAY

Sir Joseph Hooker has addressed the following letter to Sir Trevor Lawrence in reply to the congratulations of the Royal Horticultural Society on the occasion of his ninetieth birthday:

The Camp, Sunningdale,
July 15, 1907.

My Dear Sir Trevor: Your letter of the 25th June conveying the hearty congratulations of the President, Council, and Fellows of the Royal Horticultural Society on the approach of my 90th birthday has gratified me more than I can express.

It is not by many times the first instance I have experienced of the friendly and all too liberal estimate of my labors in the cause of horticulture that the society has entertained.

It has been a source of great regret that I was obliged, when resigning my post of chairman of the Scientific Committee, to abandon all hope of attending our meetings on account of having to devote my energies to the Directorship of Kew, and to the completion of labors on botanical works I have in progress.

I had also to endeavor to overtake arrears of work extending over many years, which are still far from being overtaken. As a botanist I have hereby lost much, for since the days of David Douglas, the Royal Horticultural Society has contributed more botanical science, as represented by collections, publications and experimental research, than any other establishment in Europe.

I have now to request you as their president to accept yourself, and convey to the council and to my fellow-members, my pride and gratitude for this most welcome evidence of their friendship and esteem.

With every good wish for the continued welfare and renown of the society,

Believe me, dear Sir Trevor, sincerely yours,
Jos. D. Hooker

SCIENTIFIC NOTES AND NEWS

The seventh International Zoological Congress opens its meeting under the presidency of Mr. Alexander Agassiz at Boston on August 19. An account of the general features of the program, including the visits to New York, Philadelphia and Washington, will be found in the issue of Science for May 17. The full program, so far as papers were announced up to that time, will be found in the issue of Science for August 2.

The British Association for the Advancement of Science opened its annual meeting

at Leicester on July 31, when Sir E. Ray Lankester resigned the chair to Sir David Gill, who gave the address published in this issue of Science. The nomination of Mr. Francis Darwin to be president next year was confirmed.

Dr. J. S. Murat has been appointed director of the Meteorological Institute of Bucharest in the place of Dr. S. C. Herpites, who has retired from active service.

Dr. Otto Wallach, professor of chemistry at Göttingen, and Dr. Karl Graebe, professor of chemistry at Frankfort, have been elected corresponding members of the Berlin Academy of Sciences.

Professor Eugene S. Talbot, professor of stomatology in the Woman's Medical School of Northwestern University, is one of the honorary presidents of the International Stomatological Congress that met in Paris last week.

Dr. Hugo Bücking, professor of mineralogy and petrography at the University of Strassburg, has celebrated the twenty-fifth jubilee of his university professorship.

Dr. Friedrich Hildebrand, professor of botany at Freiburg, has retired from active service.

Professor J. S. Kingsley, of Tufts College, spends next year in Europe on leave of absence, sailing on August 31. His address for letters will be in care of Baring Brothers, Bishopsgate, London, England. Separata, etc., may be sent as usual to him at Tufts College, Mass.

Professor B. K. Emerson, of Amherst College and the Geological Survey, is this summer continuing his studies in the geology of central Massachusetts. The Taconic Quinsigamond and Ware folios are practically ready for publication.

Professor Jeremiah W. Jenks, of Cornell University, a member of the United States Immigration Commission, is on a tour of the Canadian northwest investigating the matter of American immigration into Canada.

Professor T. G. Masaryk, professor of philosophy in the Bohemian University of Prague, has arrived in New York. He will make an address at the International Council of Unitarians, which meets in Boston in September, and will also make other addresses.

A monument to Bunsen is to be erected at Heidelberg.

Dr. Ernst Kayser, the astronomer, has died at Danzig, at the age of seventy-eight years.

Dr. Heinrich Hoyer, emeritus professor of anatomy at Warsaw, has died at the age of seventy-two years.

The death is announced of Dr. H. Kreutz, professor of astronomy at Kiel.

Dr. Walter von Knabel, docent for geology and paleontology at the University of Berlin, died while on an expedition to the interior of Iceland.

Professor Willoughby Dayton Miller died on July 27, after an operation for appendicitis, in the hospital at Newark, Ohio. Dr. Miller took his A.B. degree at Michigan in 1875. He studied dentistry at the University of Pennsylvania, where he graduated in 1879. He then went to Berlin, and became a professor in the dental department of the university of that city. He held this position until last month, when he resigned it to accept the deanship of the dental department of the University of Michigan. He returned to this country in June, and after a few days spent in Ann Arbor in arranging for the duties which he was to assume in September, he went to his old home in Ohio. Dr. Miller is well known, both among medical and dental men, on account of his most excellent and thorough work on dental caries. His great book entitled "Die Bakteriologie des Mundholdes" has been translated into several modern languages. His death will be greatly regretted both in this country and in Europe.

The hygienic exhibition which is to be held in connection with the fourteenth International Congress of Hygiene at Berlin in September will be under the auspices of the Cultus-minister, the Imperial Health Bureau, and the medical departments of the army and navy, as well as leading representatives of

hygienic science in general. The president is Professor Radner.

The eleventh International Navigation Congress is to be held at St. Petersburg in May, 1908.

The third International Congress on Provision for the Insane will be held at Vienna from October 7 to 11, 1908, under the presidency of Professor Obersteiner. The general secretary of the congress is Dr. Alexander Pilcz, ix Lazaretthasse 14, Vienna.

It is stated in *Nature* that the *Nimrod,* in which Mr. E. H. Shackleton's expedition will proceed to the Antarctic regions, sailed from the Thames on July 30 with Lieutenant Rupert England in command. Lord Kelvin has presented to the expedition a standard compass and sounding instruments. The admiralty is lending a compass, chronometers, charts and sounding apparatus, as well as three Lloyd-Creak Dip instruments for the landing party. Watches are being supplied by the Royal Geographical Society, and, in addition, the vessel will be equipped with a liquid steering compass and a special pole compass. The members of the expedition on board of the *Nimrod* are Mr. James Murray, the biologist of the expedition; Mr. W. A. Michell, surgeon and zoologist; and Mr. A. F. Mackay, the junior surgeon of the landing party, who will also engage in zoological work. At Lyttelton, New Zealand, the remaining members of the expedition will join the ship. These include, besides Mr. Shackleton, Mr. E. Marshall, senior surgeon of the shore party and cartogapher of the expedition; Lieutenant Adams, R.N.R., who will be in charge of the meteorological work; and Sir Philip Brocklehurst, for survey work and field geology. Dr. David, professor of geology in Sydney University, has arranged to accompany the expedition south to King Edward VII. Land.

English journals state that of eight balloons sent up by the staff of the Manchester University in connection with the international movement to discover data in regard to the atmospheric phenomena of the clouds, three have been located. The balloons carried a recording apparatus, bearing instructions to the finders to return them to Manchester, and the examination of the records, which were picked up at Macclesfield, Lincoln and Leeds, shows that the balloons reached an altitude of about ten miles. A temperature of 20 degrees below zero Fahrenheit is recorded.

Although diamonds have been found in at least thirty places in the United States, the only locality where they occur in place has recently been discovered and has been investigated by Mr. George F. Kunz, the gem expert, and Dr. H. S. Washington, petrographer. They occur in an igneous rock, similar to that of the South African mines, which forms a small stock near Murfreesboro, Pike County, Ark. The first two stones were found August 1, 1906, and since then many of them have been picked up, the total number found at last report being 130. Many are white and of good water, others are yellow and some are of brown bort. The two largest stones weigh 6½ carats, one being exceedingly fine white and the other brown. They are found on the surface as well as within the greenish, friable, decomposed peridotite, a rock somewhat like the famous "blue ground" of Kimberly. The property is being actively prospected and developed.

Nature states that private enterprise has succeeded in founding, with the sanction of the Ministry of Education, confirmed by the Czar, an Institute of Archeology and Archeography in Moscow. The institute, which has just obtained its charter, ranks with a university, and is open to all graduates of Russian or foreign universities. Its aim is to prepare qualified archeologists and "archeographists." The latter term is applied to persons skilled in the preservation and use of historical archives, libraries, museums and other collections, public and private, demanding special knowledge. The Moscow Institute of Archeology is the first institution in Russia founded on autonomous principles; it has the right to elect its own staff of professors, and generally to conduct its own internal affairs, subject only to a possible veto of the minister of education in certain cases. The course is a three years' one, the final year of which

must be spent in practical work either in archeological expeditions and research among the monuments of antiquity as yet so little studied in Russia, or in similar special work at home or abroad. The institute grants the degree of doctor of archeology or archeography. Among those connected with the new institute whose names are favorably known outside Russia may be mentioned Dr. Uspensky, director of the institute, the author of fifty capital monographs in Russian; Dr. Fleischer, who was associated with English and American archeologists in recent excavations in Persia; Professor Grot, and other Moscow professors. Docent Visotsky has been appointed secretary to the institute.

UNIVERSITY AND EDUCATIONAL NEWS

The Kansas legislature appropriated for the state university at its last session $250,000 for the erection of engineering buildings, work on which will be begun at once.

Through the generosity of Mr. Arthur J. Cox, of Iowa City, an alumnus of the engineering department of the State University of Iowa, an annual prize of one hundred dollars has been established in the College of Applied Science of that institution for the best thesis submitted for the first degree in engineering. The prize is to be known as the "Thomas J. Cox prize in engineering," in memory of the father of the donor.

According to Consul-General T. St. John Gaffney, of Dresden, during the winter 1906–7 the twenty-one universities of Germany were attended by 45,136 students, of whom 254 were women. He gives the following details: The increase over the corresponding term of last year is 2,740 students. In addition to these numbers, 5,509 persons availed themselves of the privilege of listening to lectures without matriculating as members. Of this class 2,105 are women. As regards the various courses, the figures give the total number of Protestant students of theology as 2,208 and of Catholic, 1,708. The number of students of law is given as 12,146, of medicine, 7,098; of philosophy, history and languages, 10,985, and of mathematics and natural sciences, 6,234. The largest increase of students has taken place in medicine and philology, while there is a continued scarcity of Protestant theological students. The best attended university is that of Berlin, with 8,188 students; next to this comes Munich, with 5,567; Leipzig, with 4,466; Bonn, with 2,992; Halle, with 2,250, and then Breslau, Göttingen, Freiburg, Strassburg and Heidelberg. The two last have improved their position in the tabulated list of attendances, whereas Tubingen, Giessen and Erlangen, which are favorite universities in summer, take lower places in the list than formerly.

Dr. James E. Talmage has resigned his position as professor of geology at the University of Utah in order to devote himself to investigation in the field of mining geology. Professor Talmage has occupied the chair since its establishment as an endowed professorship thirteen years ago. In 1897 he retired from the presidency of the University of Utah to continue his work in geology. His successor in the department of geology is Dr. Fred J. Pack, who is one of his former students and a graduate from Columbia University, now professor at the Brigham Young College.

Dr. Frederick Hollister Safford has been promoted to an assistant professorship in mathematics in the University of Pennsylvania, and Messrs. Maurice J. Babb and Louis O'Shaughnessy have been appointed instructors in mathematics.

Dr. Gustav Hellmann has been appointed professor of meteorology at the University of Berlin and director of the Meteorological Institute, in succession to Professor W. von Bezold.

Dr. Cornelius Doelter, of Graz, has been appointed professor of mineralogy in the University of Vienna, in the place of Professor G. Tschermak, who has retired.

M. H. le Chatellier, of the Collège de France, has been appointed professor of general chemistry at the Sorbonne, in succession to Moissan.